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THE  
**Philadelphia Journal**

OF THE  
**MEDICAL AND PHYSICAL  
SCIENCES.**

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AND

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**VOLUME XI.**

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**VOL. II. NEW SERIES.**

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PHILADELPHIA:  
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1825.

# January 1900

1. The first of the month was a fine day, with a light breeze from the west and a clear sky. The temperature was in the 50s.

2. On the 2nd, a heavy fog came in from the sea, and the day was very dull. The wind was from the north-east.

3. On the 3rd, the fog cleared away, and the sun shone brightly. The wind was from the south-west.

4. On the 4th, the weather was fine, with a light breeze from the west. The temperature was in the 60s.

5. On the 5th, a heavy rain fell, and the day was very dull. The wind was from the north-east.

6. On the 6th, the rain continued, and the day was very dull. The wind was from the north-east.

7. On the 7th, the rain continued, and the day was very dull. The wind was from the north-east.

8. On the 8th, the rain continued, and the day was very dull. The wind was from the north-east.

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11. On the 11th, the rain continued, and the day was very dull. The wind was from the north-east.

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31. On the 31st, the rain continued, and the day was very dull. The wind was from the north-east.

## TO READERS AND CORRESPONDENTS.

By the recent visit of one of our publishers to Europe, extensive arrangements have been made to obtain for this Journal a regular supply of the most esteemed medical and scientific periodicals published in France, Germany, Italy, &c. as well as the new medical works which from time to time may appear in those countries. We shall thus be enabled to present our readers with the earliest information of their contents, without having to wait until such intelligence reaches us through the British press. That the latter is a very slow and circuitous channel is well known, from the circumstance of some of their journals obtaining the first notice of some continental works from our pages.

In an Essay on Yellow Fever, in one of the preceding numbers of this Journal, in which reference was made to a communication from Dr. Chervin, in conversation with one of the Editors, while in this city, the following inaccuracies occur as regards the report of his statements. Though not of much importance, we take the earliest opportunity of correcting the errors.

1. Dr. Chervin was not commissioned by the French government, as we apprehended, to visit the countries of Yellow Fever, with a view of exploring the nature and causes of that disease, but engaged in the undertaking voluntarily, and of his own accord.

2. That he had received 500, and not 557 documents, in relation to the subject of the disease.

3. That in his circuit through the southern section of the United States, he did not visit the City of Richmond.

4. That he ultimately found among the Physicians of Philadelphia, six *non-contagionists*.

5. That he met with three at New Haven, two at Middletown, one at Providence, three or four at Boston, and one at Portland, entertaining the same opinion in relation to the cause of the disease.

The paper of our friend Dr. HARRISON, of Louisville, Ky. which has been hitherto unavoidably postponed, will appear in the next number.

We shall be pleased to receive from Dr. PERRINE, of Natchez.

Mississippi, further communications of the interesting results of his experience. His extensive opportunities, and cultivated talent for observation, cannot fail to render his practical remarks of value to the profession.

The notice of Dr. LOBSTEIN's work on Phosphorus is deferred until our next number.

We congratulate the American Profession on the important acquisition to the *Materia Medica*, which is promised by the successful exertion of Mr. GEORGE W. CARPENTER's chemical researches. The discovery of an Alkaline Principle obtained from the bark of the common *Dogwood*, possessing similar, if not superior virtues to the celebrated and expensive *Quinine*, may be considered in this country an invaluable addition to our stock of remedial agents. Dr. MORTON's communication, given at the end of this number, will be read with great interest, and we hope hereafter to present from the same competent hand, a full statement of the process by which this medicine is to be obtained.

In order that the appearance of their papers may not be unnecessarily delayed, our distant correspondents are requested to forward their communications by mail, as soon after the publication of a number of the Journal as possible. Arriving just at the end of the quarter, they can rarely or never be introduced in the forthcoming number, but must lay over for three or four months longer.

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THE

# Philadelphia Journal

OF THE

## MEDICAL AND PHYSICAL

## SCIENCES.

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ART. I. *Recapitulation of the Instructions, given to Females during Pregnancy, Labour and Confinement; as well as Rules for the general Management of the Child during the Month.* By W. P. DEWEES, M. D.

[Selected from his recent work on the Diseases of Children.]

TO prevent abortion, &c. she should scrupulously observe the following rules.

1. She must avoid all unnecessary, and especially violent exercise, or exertion; such as too fast walking, running, dancing, &c.

2. To avoid, as much as may be, placing herself in a situation which may subject her to unpleasant sights or seeming dangers.

3. To shun over-heated rooms, and stimulating liquors of every kind.

4. To avoid all substances that have a tendency to produce a costive state of bowels; or those which may give rise to "indigestion," as late suppers, too great a quantity of food, &c. &c.

5. To take no substance, or drug, that shall give too frequent, and too severe motion to the bowels; or such as shall too severely constipate them, as chalk, opium, &c, and especially not to disregard the calls of nature, when they would be successfully exerted.

6. To remove from her chest, waist, and abdomen, every restraint; lest undue pressure should be made upon them.

7. To avoid all substances that may have a tendency to increase the irritability of the system; as strong tea, coffee, opium, &c. the too long indulgence in bed, &c.

8. To shun all severe study, night-watching, &c.

9. To avoid with much care, unnecessary blood-letting, or submitting to this operation, merely because she be pregnant.

10. To have however recourse to this operation, when pain, head-ache, a sense of fulness, giddiness, the loss of, or imperfect sight, &c. may be present, to declare the necessity of it—but it were always better when practicable, to have the advice of a physician.

11. The woman who may be in the habit of miscarrying, should never venture upon blood-letting without advice, as it sometimes produces the evil it is intended to prevent.

12. To avoid the indulgence of all inordinate appetites; as too much repletion of the stomach, may give rise to many formidable diseases.

13. She must give up the false notion, that more food, &c. than ordinary, is required, because she is pregnant, as the opinion is not founded in either reason or experience.

14. She must assure herself, by a reliance upon the opinions of those whose business it is to ascertain the truth upon this point, that nature institutes the sensation of nausea, and the act of vomiting, with a view to prevent too great fulness, during this period.

15. She must not indulge in the fear, that should a certain longing have been ungratified, her child incurs the risk of being marked in consequence of the disappointment, as this apprehension is not warranted by reason, nor confirmed by experience.

16. She must dismiss from her mind all apprehension of

future consequences to her child, should she have been so unfortunate as to have suffered any great alarm; been surprised by any unexpected event, or appalled by some frightful object or occurrence—as there is no one good reason to believe in such influence upon the child.

17. To most sedulously guard against any sudden gusts of passion; as any indulgence in them, may be highly injurious, if not fatal to herself and child.

18. To solicit as much as possible, tranquillity, and equanimity; as their influence is highly valuable to herself and child.

19. To believe, in general, that food injures more by its excess than its quality; by taxing the powers of the stomach too highly.

20. To avoid with as much care as she would impending ruin, an indulgence in cordials, liquors, &c. under the pretext, of calming sickness, relieving pain, or expelling wind: as a destructive habit is but too easily generated by their employment.

21. She must not persuade herself, it is only the excessive indulgence in such articles, that is mischievous; but she must clearly understand, that every thing which may unduly stimulate the system, is highly injurious to the pregnant woman.

22. Let her turn a deaf ear, to every tale of disaster, or of horror, which purports to have happened to the pregnant, or lying-in woman—for upon investigation, such tales will almost always be found without foundation, or very greatly exaggerated.

23. Let her procure the best aid for the period of her necessities, that circumstances will permit.

24. Let her not be imposed upon by a false theory, or bad advice; nor use too much exercise towards the latter period of her time, lest she provoke premature labour.

25. Let her not, when the period of labour is approaching, indulge in gloomy forebodings, or in unreasonable fears for the event; nor by any means whatever, forget how rarely death happens during, or after a well-conducted labour.

26. Let her not attempt to increase the frequency or force of her pains, by taking improper articles of food, or by frequent marchings across the floor.

27. Let every precaution be taken against an attack of fever; for this purpose she must scrupulously avoid every stimulating substance, either as food, drink, or remedy. Let her bear in mind, that an overcharged stomach is always unfavourable to the healthy progress of labour, and to its eventual safety.

28. To prevent despondency, because the labour may be rather longer than she anticipated; let her recollect that the safety of this process does not depend upon the celerity with which it is performed.

29. That she may secure to herself the best possible chance for her safety, let her have no opinion of her own, that may clash with those of her medical attendant; let her therefore be passive, and obedient.

30. Let her not, as she values her life, indulge in any gust of passion; lest she provoke incurable convulsions.

31. Immediately after she is a mother, she must impose upon herself the most perfect tranquillity; that no untoward circumstance may be provoked.

32. She must carefully avoid all the exciting causes of fever, as far as in her power, by not indulging in improper articles of diet; sitting up too early and too long; too hot a room; curtains too much closed; or seeing too much company.

33. She must aid the exertions of her physician, to prevent any after evil; by implicitly following his directions, and preventing, as much as may be in her power, the nurse running counter to them, especially until after the fifth day complete.

34. Let her not permit herself to be persuaded from having her child put to the breast; so soon as she herself may be able to bear the fatigue.

35. Let her not delegate to another the sacred duty of nursing her own child; unless the reasons for so doing, are insurmountable.

36. She must most scrupulously attend to the dressing and undressing, or even performing this herself; when her health and strength will permit this delightful task.

37. She should never, under any pretext of convenience, permit her child to be fed, so long as she can supply it with sufficient nourishment herself; and to secure to herself this important point, she should pay a scrupulous regard to her diet, and her exercise in the open air.

38. The dressing of the child should early engage the attention of the mother; it should always be made subservient to comfort instead of show.

39. This should consist in part of flannel; especially during cold, or even cool weather.

40. The dressings of the child should be changed as often as they become soiled; when this luxury can be indulged in.

41. Though it may be found, that flannel is the most eligible substance as a general rule; there are cases in which it may be improper; and these should be carefully distinguished.

42. The belly band is one of the most important parts of the child's dress; it should *always* be made of flannel, and should be always cut *bias*.

43. The greatest care should be taken to apply it properly; and too tight an application should be particularly avoided.

44. If this bandage be applied too tight; it may produce the evil it is intended to prevent.

45. The child should be carefully protected against all unnecessary wet; and when it is discovered to be in this situation, it should be changed as quickly as possible.

46. In dressing the child, as few pins should be employed as possible; three are all that are necessary, if the clothes be properly arranged.

47. As a general rule, the child should never be fed at this period of its life; there may be exceptions however, that may render this necessary.

48. When an exception to this rule exists, the food of the child should consist, of milk, water, and sugar; and exhibited under the important restrictions we have made.

49. If the child be fed, it should have but small quantities at a time, and never crammed to surfeit.

50. The woman should exercise in the open air, as soon as this is rendered safe by the state of her health, the period after confinement, and the state of the weather.

51. She should never subject herself to partial exposures, under the hope of "hardening herself."

52. The reapplication of a diaper after it has been dried simply, should be prohibited; as several evils arise from the practice.

53. The mother may indulge in such diet, as her experience has proved to be innocent—for it will seldom or never disagree with the child, when it perfectly suits the mother.

54. When the mother experiences any inconvenience from the use of any substance or substances; she should not hesitate a moment to abandon them; for if they disagree with her, they will be sure to do so with the child.

55. Should there be a failure in the quantity of milk; the mother must not attempt to increase it, by highly seasoned food, and stimulating drinks.

56. She may however use with great advantage sometimes, the nutritious substances recommended in the text.

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[From the same Work.]

## OF THE NURSERY.

**EVERY** body almost, in easy circumstances, has a part of the house appropriated to what is called the "Nursery." The part selected for this important office, is generally, or at least too often the most exceptionable part of the building. It is usually selected from the other rooms because it is "handy," or because it is the only one that can be spared; without the smallest attention being paid to its fitness for the purpose for which it is designed.

The room selected for the purposes of a nursery, should have every advantage which space or location can give, when

these can be commanded. We shall therefore say in what a well-appointed nursery should consist, leaving it of course to the discretion of every individual to approach it as nearly as circumstances may permit.

The nursery should be spacious, with a high ceiling, and perfectly dry; that is, it should not be exposed to the operation of any cause that may render it damp; as on a ground floor; too much shaded by trees; or placed beyond the occasional influence of the sun. Its windows should be tight, and the walls dry; the floor should be of wood that will quickly dry, after being wetted for the purposes of cleanliness; but the utmost care should be taken, not to hasten this process, by placing ignited charcoal in its centre. Serious mischief has frequently been done, by this absurd and dangerous practice.

It should be so situated, that the door or doors shall not open immediately on staircases; or should this unavoidably be the case, the heads of the stairs should be secured by latticed half-doors, and these so constructed, by having their slats placed perpendicularly, that the child cannot climb upon them, and thus defeat their object.

The windows should have cross-bars placed before them; they may be five inches distant from each other, that they may be opened without apprehension for the purposes of ventilation, or air, and without danger of accident to the child. They however should have shutters, that the room may be darkened, when the abstraction of light becomes necessary. The windows should not however have curtains of a glaring colour, as the effects of the light will be increased thereby, and prove injurious to the eyes of young children.

If possible, the nursery should consist of two rooms, opening into each other; this would yield very great advantages; the children could retire to one, while the other was ventilating, or getting cleaned, by washing or sweeping; which would contribute greatly to their comfort, as well as to their health. They would also be removed from the dangers of damp, the inconvenience of dust, the risks from a cold stream of air, while the room is drying or sweeping, besides having an enlarged space for the exer-

cise of their limbs. In a space so extensive as this, they could improve their strength by engaging in many little sports which children are so ingenious to devise, when they cannot take exercise in the open air, from the condition of the weather.

Besides, such an arrangement will permit the children to have a room fresh and sweet in the morning after having rendered the other foul by sleeping in it. This is an advantage which has been but too little attended to, notwithstanding its obvious utility, especially to the younger children, who cannot always escape into the fresh air in other parts of the house.

Carpets in cold weather are decidedly useful, if they be properly managed; that is, well shook, and aired, every week. By this means, the dust is removed from them, and they have the advantage of becoming dry by exposure. We are sensible that several objections may be raised against carpeting a nursery; as the great quantity of dust they accumulate; their becoming often wet, without the chance of drying; their retaining grease so fixedly as not to be removed; their absorbing, and then giving out a variety of impurities, &c. But notwithstanding all these reasonable objections, we are persuaded, that one single advantage which they possess in a nursery, overbalances all that may be said against them—namely, their protecting the heads and limbs of children from injury when they fall.

The elastic material, (wool,) of which the carpet is formed, is well calculated to break the force of the blow which the head or limbs of the child receive, when it falls upon it; so much so is this the case, that we have not known a single instance of serious injury from falls upon them. We are sure to have our anxiety diminished, when called to a child who has received a blow upon the head from a fall, when we are informed, it fell upon a carpet. We are therefore of opinion, that when the child falls from a moderate height, it will rarely, if ever, suffer a serious injury from it—this however would not be the case, did the child fall upon the naked floor.

We must however be understood to recommend carpets in cold weather only; for so soon as the weather becomes

sufficiently warm to do without fire, the carpet should be removed, and its place supplied by an even, well-stretched mat; or the floor may even be left bare; for at this period, it is to be presumed, children will be but little confined to the nursery, unless the state of the weather prohibits their enjoying the open air; this state of weather may consist in its being wet, too windy, or too hot.

The furniture of a nursery should be as little in quantity as convenience will permit, that the children may have the space that would be unnecessarily occupied by many articles; especially chairs and tables. It should therefore consist of the beds for the children and nurse, or we would rather say mattresses, as we are of opinion that feather beds should be driven from the nursery, for the following reasons—first, they are too warm for the purposes of the best health, especially with feeble children; accumulating so much heat, as to unduly stimulate the whole cuticular system; thus giving rise to unnecessary, nay injurious perspiration; second, the effluvium from feathers is extremely oppressive, particularly in warm weather, and to children of feeble lungs; third, they discharge a prodigious quantity of dust, intermixed with minute portions of down, occasioning cough, and other inconveniences.

If it be objected, that mattresses are too cold in our climate for winter, we would immediately obviate it, by recommending the spreading of a blanket over the mattress, which will effectually remove the inconvenience complained of.

When practicable, children should sleep in separate beds; and these should be large; for it is injurious to have them cramped when they sleep, as well as indelicate to crowd opposite sexes together. Besides, the degree of heat generated by contact, will be certain to make them uncomfortable; they will throw off the bed clothes, and thus expose each other to colds.

Children should never have more bed clothes spread over them, than is sufficient to maintain a proper degree of warmth; if more be put upon them, they become oppressed, or perspire; both of which should be avoided.

Should we not however succeed in establishing our objections against feather beds in winter, we are persuaded every body will agree in the use of mattresses during hot weather. Should these not be at command, the sacking-bottom, or even the floor, should be substituted—for almost any thing is preferable to feathers.

It is in the nursery, in a great measure, that the habit of early or late rising is generated—this is a matter of much importance; and the greatest regularity should be observed, that a proper one is formed. Children should therefore retire at a regular, and sufficiently early hour, to ensure their early getting up; for beyond a certain time, sleep is injurious. It would however be a little difficult to establish a positive rule upon this subject, as some children, like adults, will require more sleep than others. Children who exercise much, will need more sleep than those who exercise but little; consequently they should not be confined to precisely the same number of hours.

All children are disposed to be early risers; this propensity should therefore be cultivated, by permitting them to retire sufficiently early to bed; and after they are in bed, they should not be allowed to keep each other awake by playing, and thus depriving themselves of sleep: for the same reason, no noisy employment should be permitted in the nursery, that the children need not be disturbed. Indeed it would be best, when children have attained their third year, or even before, that they should not be allowed even light in their rooms, that they need not unnecessarily be kept from sleep, as well as to prevent any apprehension from being left in the dark.

When children first awake in the morning, however early this may be, provided it be after daylight, they should be allowed to get up, and be dressed; for if this be not done, and they are forced to lay longer than is pleasant to themselves, they will become fretful and dissatisfied, or again fall to sleep—in either case, a real evil is induced; in the first, the disposition of the child is injured, and in the second, a habit of lying too long is generated.

It should be carefully guarded against, that no unnecessary habits are indulged in during the period set aside for sleep such as drinking water several times in the night, or rising too often to discharge the contents of the bladder. If the first be indulged in, an artificial thirst will be created; if the second, the bladder acquires a preternatural degree of irritability, which is almost sure to terminate in the disgusting and inconvenient habit of wetting the bed.

After children have risen from their beds, they should be dressed as quickly as possible; they should be carefully washed, and combed, and then be permitted to inhale the fresh air, either in doors or without, with as much freedom; as the nature of things will permit. For the first purpose, the nursery should be well, but carefully ventilated; or what is still better, the children should be allowed to retire to another room when practicable, and especially during the time the nursery is cleaning; hence, the propriety of two rooms being devoted to this purpose; and for the second, when the weather is proper, they may be allowed to go out of doors.

So soon as the above necessary operations are performed, children should have their breakfasts, so that the stomach need not suffer, either from too long fasting, or from the indulgence of too great an appetite, excited by a long abstinence.

A cradle for young children is a very important appendage to a nursery, notwithstanding the objections which have been made against it by ingenious speculators upon the subject of the physical education of children. The advantages of the cradle are, 1st, it can be placed in any situation of the room, without disturbing the child, for the advantage of either warmth or coolness; for light or darkness, or for air; 2d, it supplies the most gentle and certain anodyne, if we may so term it; since it will amuse by its motion when the child is first placed in it awake, lull by its sameness when disposed for sleep, and perpetuate it when desirable by a familiarity with its action: for it must be recollected, that for nine months previous to birth, the child has been indulged in the gentlest motion, in the fluid in which it constantly swims; conse-

quently, the motion of a cradle would seem to be but a continuation of an exercise it had been long used to.

The objections to the use of the cradle are easily obviated. It is said, it may produce fatuity, by constantly shaking the brain; this could not possibly happen, unless the cradle were violently agitated; in which case, it would be the abuse of the cradle that should be objected to, since no such consequence could possibly follow its proper use: for did gentle agitation do mischief to the organization or functions of the brain, why are not all children born fatuitous, since this organ is subjected to it from its earliest formation? Another objection is urged, which is as easily obviated; it is said, the child runs much risk by its liability to upset—now, it must be by the employment of extreme violence, or carelessness, that this can be rendered an objection to the cradle; for certain it is, the proper use of this machine can never be attended by such a consequence.

In using the cradle, however, we would suggest certain precautions, that it may not be converted into an improper machine. We would forbid all violent motions of it, since it would not only defeat the objects for which it is employed, but might be attended by the risk of upsetting. The motion of the cradle should be made an efficient means to procure rest; and should therefore never be used so constantly as to lose its effects, by too constantly employing it; nor should its influence ever be taken advantage of, to procure more than the necessary degree of sleep, as it may tend to the disadvantage of the child; nor should we think the cradle necessary to children much beyond the second year, as at this time, their exercise will dispose them to sleep soundly without its agency.

The means by which the nursery is warmed, is not a matter of indifference. Two important objects should always be kept in view in constructing it—namely, security from accident to the children; and secondly, its affording sufficient warmth.

These may be secured, by an open fire of wood, or of coal, protected by a high and substantial fender of wire, that the

children may not approach it too nearly ; or by a stove placed near the hearth, and defended by an iron railing. We however should never advise the stove, where an open fire can be made use of with equal advantage as regards heat ; and we believe that the introduction of the Lehigh coal will secure to us this desirable end, with less expense and more security than any other method.

The objections to a stove are numerous, and deserve a serious attention—1st, there is great danger that the children may get severely burnt, as we have often witnessed ; 2d, they are almost always too much heated ; 3d, the air is rendered too dry by a destruction of its moisture, and becomes impure by the burning of millions of the little particles which are constantly floating in the air ; and though one of these objections may be partially obviated by placing water upon the stove, yet it will not remove the second ; 4th, the air is almost constantly injured by substances thrown upon the stove, as grease, meat, &c. ; 5th, there is always a temptation to do some kind of cooking upon or in a stove, to the annoyance of the comfort, or the injury of the health, of its little inhabitants, beside the serious risk of scalding them by heating water upon it. We lately saw an instance of death from this cause, and have witnessed many times minor accidents from the same source.

We have already forbidden the performance of a number of operations in the nursery—we need not repeat them here—we shall merely reiterate, that the nursery should be the purest place in the house, as well as the one in which the children should most delight to be. It should therefore never be made a place of punishment, by banishing children to it, for any little delinquency, or inadvertence they may have been guilty of—but on the contrary, a temptation should constantly present itself in the nursery, by making it the place of amusement ; children will then bear being placed there, without considering it a place of confinement, or one in which they are to experience privations.

Among the proper provisions of a nursery, we should reckon a small backgammon table, with men, but without

dice. Children, so soon as they are capable of comprehending the subject, should be taught draughts, or checkers. This game is not only highly amusing, but is also very instructive, as it calls forth the resources of the mind in the most gentle, as well as in the most successful manner. It becomes a source of endless amusement; it never tires, and it always instructs.

Battledore, or shuttlecock, is also a proper game for the nursery; this gives great agility, as well as great vigour to every muscle in the body. It exercises with but little fatigue; it gives great practical accuracy to the eye, and to the hand; while the mind is agreeably amused. A large cup and ball should be added to the above articles, as it affords great opportunities for skill, as well as excites an agreeable emulation to excel.

A rocking-horse of a good size should also be an appendage to a nursery—this article however should be considered as a luxury, or it will become abused by becoming too familiar; it should therefore only be introduced occasionally, and that as a reward for good conduct.

Slates, and pencils, afford much employment, as well as amusement to children—giving them the habit of making letters and figures very early, as well as calling forth their imitative powers in rude attempts to copy many of the objects of nature, or of art, which may present themselves. For the same reason, we would indulge children in the use of paper, and lead pencils.

To children of proper age, dissected maps, and Chinese puzzles or trigrams, are very acceptable, and highly useful: they exercise the memory, elicit ingenuity, excite a laudable ambition, and give the habit of patience and perseverance, in their various attempts to correctly dispose of the one, or discover the various combinations of the other.

A quantity of regularly shaped pieces of wood, of various sizes, should also be given to them; these they will arrange, and that sometimes with great ingenuity, into houses, temples, churches, &c. and thus become sincere admirers of each other's skill, in forming these mimic, but evanescent build-

ings; the destruction of which they ardently wish for, the instant they are formed, and whose sudden demolition the moment after, by a stroke of the hand, affords the highest gratification, because they can construct another with almost as much celerity.

Nine-pins and balls of proper size, afford great pleasure to children, and serve to divert, as well as to exercise them; for both of these are highly important, when the weather prevents their being taken in the open air. By affording them opportunities to exercise their skill, they forget they are confined, and thus is prevented that peevish discontent, called ennui, by which they are sure to be assailed, when they have nothing to employ their muscles, or to excite their minds.

We might enumerate a number of other sources of amusement to children, but every parent almost will supply the deficiency, as the necessity for variety may present itself. We do not however approve of sharp-edged or pointed instruments for children—they can serve no possible use, and may be productive of serious mischief. We have known the loss of two eyes from pointed instruments, and a number of severe wounds from sharp ones.

The nursery should be as free as possible from air-holes or crevices, that the children may not be exposed to partial draughts of air; and that the air of the room may be preserved in winter, of a pretty uniform temperature. Attention to this, will enable the children to play in every part of the room without injury; and it will also prevent the desire to crowd round the hearth, which will diminish the risk of their clothes taking fire, or doing themselves other injury.

Too much care cannot be taken to guard against the accident of the clothes taking fire; there is but one security against this, when an open fire-place is the means employed for warming the room; namely, dressing the children in worsted garments, or at least the outer ones—that is, the frocks and aprons should be of worsted. It is but too common to disregard this important precaution; and the accidents are numerous, in consequence of the neglect. Many are in the habit of dressing

their children in proper materials as regards their body clothes; but seem altogether to forget, that if these be covered by muslin or linen aprons, they may take fire, and do serious mischief; it is therefore not sufficient, that the under garments be made of woollen materials, if these be surmounted by an inflammable substance.

Children that can just run about, as well as older ones, are almost constantly in the habit of having sharp-pointed sticks for playthings; and nothing is more common than to put an end of one of these sticks in their mouth, and run with it when it is in this situation, at the risk of doing great mischief to the mouth and throat, by its being violently driven into them by the force of a fall. Our friend Dr. Physick related to us a case of lock-jaw, and death, from this cause; and we ourselves have witnessed many less severe accidents from the same source.

It is also wrong to permit children to run about the nursery barefooted, or in their stocking feet, especially if the floor be covered by either carpet or mat; as it every now and then happens that they run into their feet, needles, pins, nails, glass, or other sharp substances, that may be concealed in the substance of the floor covering.

The introduction of glass into the nursery should be avoided as much as possible, as a constant liability to accident is incurred, by its being broken, and strewed over the floor, and by the children treading upon, or handling the pieces.

We cannot recommend too earnestly, the frequent ventilation of the nursery. The best possible method to purify the air, is by the admission of fresh air from without; this should be done at least daily, by opening the windows and doors for a short time. During the process of ventilation, however, the children should be removed from the nursery, or protected by a screen, from the immediate current of air; but the former is the better plan, as children are not always obedient to the wishes of their nurse, and may expose themselves very improperly. This ventilation should not be performed if the weather be wet; as the damp air might prove more injurious, than the air intended to be removed.

No sand should be strewed upon the floor of the nursery; as it is always inconvenient to walk upon, and furnishes a great quantity of very fine-shaped dust, which is very injurious to the eyes, when the floor is swept, or it is otherwise disturbed.

Some have recommended fumigations of vinegar, and other substances, for purifying the air; this is neither necessary nor availing, especially as we can always command a much better article, namely, the fresh external air.

The air of a nursery should be maintained at a pretty uniform temperature—for this purpose, a thermometer should be a part of the furniture of every well-appointed nursery. It should be placed remote from the fire, and not to face it; as it may, if so situated, give a false result, as it will be necessarily affected by the radiant heat of the fire. It should never exceed 66 or 67° of Fahrenheit. The thermometer should be placed out of the reach of the children, or it will be quickly destroyed.

We are fully persuaded that the excessive heat of nurseries has occasioned a great mortality, especially among very young children. In the first place, it over-stimulates them; and in the second, it renders them so extremely susceptible of cold, that every draught of cool air endangers their lives. They are maintained in a constant state of perspiration, which is but too frequently checked, by an exposure to an atmosphere of even moderate temperature.

Children should never be left alone—their helpless condition requires constant care, especially when very young. They should never be left to themselves while feeding, as they may choak for want of proper and timely assistance; nor should they be placed upon a chair or table, unless they are well watched. The children of the poor very often meet with serious accidents, from the necessity the mothers are frequently under, to leave them for a time to take care of themselves. A friend, lately related to me a fatal accident arising from this cause, which may perhaps serve as a warning to those who are in the habit of leaving children

alone, when there is no absolute necessity to do so. A poor woman, who had been spinning upon a large wheel, was obliged to leave the room for a short time. Before she went, she placed her child, (an infant who could not walk,) upon the floor, some distance from the wheel. She presently heard something fall, and her child scream violently—she ran instantly into the room, and found the poor infant transfixed to the floor, by the spindle of the wheel passing entirely through its body. She supposed the child had pulled at the thread she had been spinning, which was sufficiently strong to over-set the wheel.

Animals should not be left with children when alone, for both dogs and cats, may be provoked to do mischief, if too rudely assailed by them. Cats, by common consent, are driven from the sleeping places of children, under an apprehension that they will “suck the children’s breath.” If this phrase mean any thing, it is that the cat can produce suffocation, by arresting either the ingress, or egress of the air—a thing altogether impossible, unless the animal should have sagacity enough to stop both the mouth and nostrils at the same time; for should the mouth be closed, respiration can be carried on through the nostrils; should the nostrils be obstructed, it will go on through the mouth.

When a night lamp is burned, it should be always placed in the chimney-place, or immediately before the door of a stove, that the smoke may be carried off. If this be not done, the air of the room becomes loaded with lampblack, to the great injury of the lungs.

It is not a matter of indifference, in which direction the light falls upon the child; young children eagerly seek it with their eyes, and if it do not fall directly upon the face, the eyes will be strained to the direction in which it is strongest. In consequence of this, the muscles of the eyes will contract the habit of moving them in an oblique direction, which may terminate in squinting. On this account, all objects capable of attracting the attention of the child from the vivacity of their colours, should never be presented to them sideways, or immediately over their heads.

We have already forbidden washing or ironing, or any other operation being carried on in the nursery, which shall load the air with vapour, as it will necessarily expose the children to colds, coughs, or other severe affections of the lungs.

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ART. II. *Posthumous Papers of JASON V. O'B. LAWRENCE, M. D. Prepared for Publication from his Manuscript Notes. by JOHN D. GODMAN, M. D.*

[Continued from page 269 of Vol. I.]

## CASES OF MORTIFICATION.

### OBSERVATION XXIII.

A BLACK man, about seventy-three years old, whom I visited in company with Dr. HARLAN, had a mortification of the right foot and part of the leg. Five weeks previous he had injured the great toe, and a black spot shortly after appeared, which induced his physician to apply a blister over the whole foot. When the blister was removed, the surface to which it had been applied was found black and mortified: other applications were made, but the mortification advanced, and in three weeks had extended over one-fourth of the leg. There was a line of separation then formed between the living and dead part, and remained during three succeeding weeks, without further progression; the mortified parts had no feeling, the skin cut like hollow, old, and dry leather. Dr. H. gave him some nitric acid solution to bathe the mortified portion, but it was applied too strong, and where it touched the living parts, caused much smarting and a slight bleeding. I saw some clots of blood on the mortified, just below the living part, as if they had exuded from this portion of the limb. His pulse was frequent, but of ordinary strength; skin of the arms cool and slightly moist; tongue smooth, red, and dry: he slept well at night, but his appetite was indifferent, and the fœtor from the mortified limb very

offensive. A poultice of bread and milk, with finely powdered charcoal was applied. Dr. Harlan thought the arteries of the arm were ossified, but could not discover any traces of ossification in the vessels of the foot. On the other leg was an indolent ulcer, which was infested with maggots.

At a subsequent consultation, in which Dr. HEWSON assisted, amputation of the mortified limb was determined on. The patient took gr. xxv of laudanum an hour before, and another dose immediately preceding the time appointed for the amputation. [When the old man was placed on the table, and the course of the incision had been determined on, Drs. HARTSHORNE and PARRISH, (neither of whom had seen the case before,) expressed their belief that the operation could not succeed, as the patient would lose at least a pint of blood before the vessels could be secured, as the pressure on the femoral artery would not entirely command the flow of blood, and the ossification of the vessels would endanger secondary hemorrhage. Dr. P. related an instance which occurred in the almshouse, of an old man with ossified arteries, who had been operated on, and the rigid vessels projected from the surface of the stump like pipe stems, and bled freely. The hemorrhage was arrested with much difficulty, but the man died.] The pulsation in the leg could be stopped by pressing on the femoral artery, where it passes over the os pubis: the pulsation at this part was very strong.

He was conveyed to the Pennsylvania Hospital, where he took a drachm of bark in a small quantity of wine three times a day with advantage. The separation of the dead and living parts was much greater than before; some of the tendons anterior to the ankle joint were exposed. Dr. B. H. Coates cut one of them, which was dead, and did not bleed, yet he could slightly flex the foot upon the leg, the muscles being sound, and the insertions strong enough. The tendo achillis was also exposed; the foot looked black; there were maggots in the mortified part of large size, and some very small ones on the healthy granulations,\* which tapered downwards very gradu-

\* Where this circumstance occurs, especially after bad compound frac-

ally from the sound skin; in many places the granulations were bloody, and seemed irritable. The mortified part was washed with the nitric acid lotion, and the living part covered with a charcoal poultice. After the mortification had been for four weeks at a stand, it invaded a small portion of the substance on the outside of the leg. As the foot was now only held by the tendons and capsular ligaments, Dr. Coates severed it from the leg; there was a very small show of blood in the parts which were not yet black. A piece of the lower end of the fibula was removed with the foot, being the fragment of an old fracture which had not united.

He continued to make use of the remedies formerly prescribed, but gradually sunk under a slow fever, which terminated in death in the eighth week from the commencement of the mortification, and in the sixth week from the time it ceased to advance.

*Dissection.* A long portion of the femoral artery of the mortified side was taken out, and was found to be filled with a coagulum, and some fluid blood. This artery had a great many pieces of ossification outside of the inner coat. After cutting off the limb, immediately above the middle of the leg, a ligature was tied very tightly on the vessel. It cut through the internal and elastic coats of the artery, and then came out of the orifice of the vessel perfectly separated like a distinct tube. A ligature was placed on the femoral artery of the mortified side, and tied very tightly. On cutting open the artery longitudinally, it was found that the ligature had cut through the internal and elastic coats, and some of the ossified portions were broken and stuck out into the cavity of the artery like loose splinters. A portion of artery taken from the right arm was not ossified, but harder than natural; but the radial artery was ossified in many spots. At the lower part of the arch of the aorta, and again lower down, there were some scattered points of ossification.

tures, it is exceedingly disagreeable, and difficult to remedy. In one instance falling under my observation, every application for the destruction of the maggots proved ineffectual, except a strong infusion of tobacco, and this proved very injurious to the patient. G.

The heart was large and flabby; the foramen ovale was sufficiently open to admit a body larger than a probe; the coronary vein at its opening into the right auricle was so large as to admit the end of my right index finger. The colour of the right auricle on its inside was deep red; that of the left ventricle was but little brighter. The colour of the right ventricle was a light chocolate with a reddish tint—that of the left auricle was the same. The mitral valves had a series of ossifications extending all along their roots; and towards the centre of the cavity of the heart, there were several cartilaginous spots. All the semilunar valves had ossifications at their roots extending some distance into the valves, being large, solid and angular, so that when the artery was laid open longitudinally at its origin and stretched transversely, these projecting points of bone with the valves stretching over and upon them, had some resemblance to bats' wings. The semilunar valves, in the systole of the heart, could not have been applied closely to the sides of the artery. Thirty hours after death the coats of the arch of the aorta separated easily from each other; first the internal, then the elastic, and then the external coat. The elastic coat was easily separated in obliquely transverse layers, which were very weak, and readily torn to pieces: the artery tied in the leg was also weak, and broke under a slight pressure. Below the arch, the aorta exhibited some ossified spots, and cartilaginous points where ossification was beginning; the ossifications below the heart were all scaly or in thin plates. The posterior tibial artery was greatly ossified on its inside, and various contractions existed in it immediately below the knee, and much higher than the mortification had extended; these contractions when forced by a probe, considerably resembled strictures of the urethra. Lower down some constrictions obstructed the artery so entirely as to prevent air from being forced through it; on cutting through the obstruction, the artery below was again pervious, and other strictures of similar character again presented, until just above the mortified parts, the artery was entirely impervious, tapering to the point where it had mortified and fallen off. On slitting the vessel to this extremity,

the internal and middle coats ended abruptly, and were consolidated, while the external hung loose in the mortified substance like a rag. All these strictures in the artery seemed to be the result of the *contraction* of the vessel; there was an appearance of some substance within, but I could not believe it any thing but the internal coat corrugated: some small branches going off from the main trunk contained coagula, but the trunk itself had none, and above the mortified parts contained some tolerably fluid blood.

From subsequent examinations I was persuaded that all the osseous points were formed in the middle or elastic coat of the vessels.

#### OBSERVATION XXIV.

*Mortification from an ulcer.*—A black, aged about twenty-two, had suffered from an ulcer on the right leg, situated over the shin, the limb was so much enlarged from a little above the ulcer, as to give an idea that the bone itself was much swelled. Being very imprudent and dissipated, his sore leg was neglected for a long time, until the ulcer acquired great size, and the limb became much swollen.

By the use of purgatives, alterative doses of calomel, adhesive plasters, and careful bandaging, the ulcer was soon reduced to the size of a quarter of a dollar, and the swelling subsided in proportion. He was never distinctly *salivated*. In consequence of some injury received in fighting or other violent exertion, it broke out again, and his master put him under the care of a quack, under whose charge he continued for some time. One night he was sent home in a cart, and was in the following condition:—Mortification had extended half way up the leg, which was nearly separated by sphacelation from the foot, at the ankle joint; the metacarpal and toe bones were nearly bare; the limb below the knee joint was œdematous. The countenance of the patient was emaciated and sunk; he had sympathetic fever with weak pulse; could speak, however, in a moderately loud voice, appearing very animated for a man in such a situation, though his liveliness was most probably morbid. The fœtor from the mortified parts was great.

The limb was amputated about two hours after his reaching home; immediately previous to the operation, his breathing, which before was short, and somewhat anxious, became still more so, and he occasionally cried out loudly. Rather more blood was lost than was absolutely necessary during the operation, because the tourniquet did not sufficiently compress the artery; yet the arterial action was so weak that a slight pressure on the mouth of the vessel, checked the discharge of blood. He cried out much during the operation and called for water. His pulse fell; forty drops of laudanum were given after the stump was dressed. He appeared alternately easy, then restless and exclaiming that he wanted air; subsequently he complained of much pain above the navel, and of pain in the head. Sometimes he talked rationally, and swore against the quack who had ill-treated him; but he was occasionally flighty. He took claret, and afterwards brandy; still his pulse sunk; he finally turned over, seemed easy and asleep, but was dead.

## OBSERVATION XXV.

*Gun-shot Wound.*—I examined the body of a physician who died in consequence of a wound received in a duel at New Orleans, and remarked the following particulars: The ball penetrated the right side about one inch above the attachment of the diaphragm to that side; after thus entering the cavity of the chest, it passed through the diaphragm about an inch from the side and wounded the back part of the right lobe of the liver, without injuring the lung. Thence it continued downwards along the right side of the spinal column, bruising the cellular substance near the kidneys very much, but wounding none of the abdominal viscera; finally it entered the second vertebra of the loins, through the upper part of its body, and thence reached the spinal canal and ascended to the middle of the eleventh dorsal vertebra, where it lay loose. The ball was rough, but must have lost great part of its velocity previous to entering the spinal canal, where it had produced no very evident injury.

The opening through the chest was valvular, the external

perforation of the integuments was not opposite to that through the intercostal muscles and pleura, probably owing to the circumstance of his arm being elevated at the time he received the wound. In passing through, the ball had made nearly a circular canal in the back and right side of the liver, and contained much coagulated blood.

His death was not the immediate consequence of this extensive wound, and it is possible that he might have recovered from the injury of the liver, as it was found united by adhesive inflammation to the wounded surface of the diaphragm. The inferior extremities were palsied; spasmodic twitches of the muscles were frequent, and his bowels costive; though as he took laudanum frequently for the removal of his spasms, perhaps the costiveness was partly owing to this medicine. He passed urine well, and had at last a diarrhœa, which seemed to hasten his death.

The direction of the ball downwards, was probably caused by the rib, while the body of the vertebra turned it upwards.

#### OBSERVATION XXVI.

A young man, aged twenty, while on a shooting excursion, fired his gun, which, being overcharged, burst in his hand. At the moment of the accident he thought his hand was slightly hurt, and was engaged in looking for the pieces of the gun barrel, when he discovered that the hand was very much shattered. The violence of the explosion nearly turned him round; he had no very acute pain, but a heavy beating sensation in the injured hand. The first joint of the index finger was torn off, as well as the end of the middle finger. The metacarpal bones of the index and middle fingers were divided to a small distance above these fingers; the palm of the hand was much contused.

On the fourth day after the accident, he rose and walked in his room, but did not talk. His pulse was slow and sufficiently full; blood was drawn from the head by leeches. Reaction of the system did not commence until from fifty-three to sixty-three hours from the injury.

He continued for several days in a condition indicative of much disturbance of the brain, without reaction, occasionally restless and moaning; and when he opened his eyes, having a vacant stare as if blind. On the seventh day he began to speak in broken sentences, and appeared rational. He continued in this state for several days, talking in imperfect sentences, generally as if deranged or delirious, often wishing to go home, and not knowing the place he was in.

On the twelfth day he was much less deranged, and for several afternoons previous he complained of head-ache. He gradually recovered his reason and was discharged cured.

In this case although the most obvious injury was done to the hand, the brain seems to have suffered severely from concussion.

#### OBSERVATION XXVII.

#### CONVULSIONS.

*Dissection.*—An old woman who had long suffered from convulsions, died after a fit of great violence. In sawing the skull which was of the ordinary thickness, the dura mater was cut, and some clear water immediately flowed, of which about an ounce was collected. This had been effused between the tunica arachnoides and pia mater and was clear, though tinged by blood from the divided vessels. The greatest quantity of water effused between these membranes was over the top of the brain.

There was some water in the lateral ventricles: the thalami nervorum opticorum adhered at the anterior part, where their colour instead of being whitish was a darkish red, and their texture much firmer than usual. When the head was depressed after raising the brain and cutting the spinal cord, much water ran from the canal, and there was also a considerable quantity between the tunica arachnoides and dura mater at the basis of the skull. The plexus choroides contained many small hydatids, as large and larger than pin's heads, in both lateral ventricles. The whole brain seemed

firmer in texture and more shrivelled than is common, and all the natural fissures were marked with uncommon distinctness. There was no effused blood within the cranium.

The abdominal viscera presented nothing remarkable; the chest was opened from the abdomen as the cartilages of the ribs were all ossified. The heart was not unnatural in appearance, but contained some cartilaginous spots at the origin of the mitral valve, indicative of beginning ossification. In the right ventricle entangled with the fleshy columns some shreds of coagulating lymph were found, which were as free from redness as if carefully washed. The pericardium contained some water.

#### OBSERVATION XXVIII.

*Diseased knee joint.*—A black man, who in his sixteenth year had scrofulous affection of the cervical glands, suffered a considerable time afterwards from pleurisy, and was subject in the spring to bad colds. Three years previous to his death, he complained of pain in the knee, which was considered as rheumatism; the knee became stiff, and afterwards swelled very much with great pain, yet he walked about for some time after his knee became affected. He always had some cough during the disease of the knee, but was free from scrofulous sores of the neck for five or six years previous to his death.

*Dissection.*—The body was extremely wasted: vast ulcers had formed on his back from long confinement to the same position, and there was a large collection of thick pus above the knee joint on the inside, extending from just above this point to more than half way up the thigh and situated in the cellular substance under the skin, and not among the muscles. There was another large abscess below the knee, on the inside of the leg between the skin and gastrocnemii muscles, and extending some distance downwards, having no communication with the upper abscess, being like it confined in a cavity surrounded by adhesions. Neither of the cavities communicated with the joint; the pus of the lower cavity did

not look so well as that of the upper, but both were free from fetor.

The patella had lost nearly all its cartilage, and the bone was denuded and rough in proportion; the remains of the cartilage were very thin, and both bone and cartilage were reddish and bloody. A plate of new thin substance separated the patella from the cavity of the joint, forming a bridge between the articulating end of the femur and tibia, (adhering lightly to the patella above and femur below, or patella without and femur within; also to the tibia,) of a reddish colour and easily ruptured. The femur weakly adhered to the tibia, the cartilage from both these bones being almost entirely gone, and the denuded surfaces having a bloody appearance. The bone was neither ulcerated nor absorbed; but there was a rough nook in the internal condyle as if some bone had been removed. The crucial ligaments were weak and easily cut through; the lateral ligaments were firm, and having on their outsides much new-formed, firm substance, resulting from inflammation: the capsular ligament could not be traced. There was no matter in the cavity of the joint; the articulating surfaces of the bone were tolerably firm; there was a partial but weak anchylosis of the joint.

The mesenteric glands were somewhat enlarged, but gave no particular evidence of having been diseased. The cavity of the pericardium contained a good deal of fluid; the heart was covered on its external surface with a layer of coagulating lymph, giving a roughness to its outside. The other viscera presented nothing remarkable.

ART. III. *On the application of Ligatures to the principal Arteries of the Limbs.* By ANTONIO SCARPA, Emeritus Professor and Director of the Medical Faculty in the University of Pavia, &c. &c.

[Translated for this Journal.]

THE solid obliteration of an artery after Hunter's operation for aneurism, succeeds more promptly when the incision is not extensive, when the vessel is very slightly separated from the cellular tissue surrounding it; when its coats remain uninjured and we only produce by the ligature a degree of inflammation sufficient to effect the mutual adherence of the arterial walls and the exudation of a plastic lymph within and without the vessel.

The adhesion of the internal membrane of an inflamed artery whose walls are kept in contact by any cause, is not different in any respect from that which takes place after simple wounds, whether this phenomenon results solely from the *inosculation* of vessels, or the effusion of a plastic lymph between the surfaces, or from both causes together. In every case nothing but a moderate inflammation should be excited, because if too violent it will destroy the action of adhesive inflammation.

In accord with these principles, practitioners have sought a simple and prompt process for securing the principal arteries of the limbs, which should occasion in a short time the solid obliteration of the vessel tied, and thus prevent the *consecutive hemorrhage*, which is one of the most serious accidents following the operation for aneurism, as practised by Hunter. Some surgeons apply a series of ligatures near to each other, and which they tie tighter the further they are from the heart, wishing thus to diminish the calibre of the arterial tube, and by consequence, the effort of the blood against the point where the obliteration is completed; others adopt the method of ÆTIUS, or rather of CÆLSUS, which consists in cutting the artery between two ligatures. Others

again prefer the *circular* ligature made with a simple thread, to that made with a flattened cord, because it is easier in tying it to rupture the internal and middle coats of the artery, a circumstance supposed very much to facilitate the adhesive inflammation and obliteration of the vessel. Perceiving the advantage of a single ligature, but thinking that the cellular membrane could not effectually resist the impulse of the blood, some place between the artery and flat ligature a little cylinder of linen covered with cerate, to avoid the rupture of the internal coats. Lately the aid of metallic instruments to produce compression, have been employed; and lastly, it has been proposed\* to use a silk thread, which is to be cut near the knot, and the wound united by the first intention, leaving the silk to be absorbed.

By repeating and varying experiments on man and other animals, I have decided on which of these methods is to be preferred: I have seen that the adhesive inflammation runs through its stages ordinarily in three or four days after the application of the ligature, which then may be untied and withdrawn from the wound; that the simplest and surest process for its production consists in the interposition of a small cylinder covered with cerate, between the vessel and ligature. PAREY, HEISTER, and PLATNER, have heretofore indicated this process. By this method, which is easily and quickly executed, although the coats of the artery remain untouched, the slight degree of pressure is kept up which is necessary to the development of the adhesive inflammation and the obliteration of the arterial tube. This process has always been successful when the artery to which the ligature was applied was free from morbid affection.

The application of several ligatures, gradually tightened, always requires a long incision; the artery must be denuded to a great extent, and the first ligatures, though moderately tightened, soon produce, and much more quickly than is generally supposed, the *ulceration* of the three coats of the

\* PHILIP S. PHYSICK, M. D. Professor of Anatomy in the University of Pennsylvania, was the first to propose the use of animal ligatures with this view.

vessel. The consecutive hemorrhage which results is an accident to which the patient is equally exposed when a ligature of *reserve* is employed that is tightened or merely passed around the artery.

Experience has also demonstrated the uncertainty of the method practised by Celsus, that is, when we apply two ligatures upon a vessel and divide it in the interval which separates them; hemorrhage frequently ensues, and when COOPER tied the carotid for the first time, he was unwilling to cut the vessel between the ligatures because he dreaded to see the one nearest the heart driven off by the impetus of the blood, as he had once observed in taking up the femoral artery in the same manner. ABERNETHEY, MONTEGGIA, ASSALINI and MORGI, cite examples of fatal accident following operations for aneurism performed after this manner, and such serious inconveniences have contributed considerably to cause its abandonment; it has been perceived that the hemorrhage when it occurs is the consequence of dividing the artery too close to the ligature, especially the one nearest the heart. This method then cannot be employed unless in parts where the artery is not deeply seated, and where we can make a long incision without fear of injuring any important part in dissecting, and insulating the vessel to the extent necessary for cutting it at a proper distance from each ligature. Thus, it is impossible to tie in this manner the carotid near the sternum, the iliac within POUPART's ligament, the axillary between the coracoid process and the sternal portion of the clavicle, or the subclavian at its passage through the scaleni muscles.

Moreover, in the cases analogous to these, where it is difficult to lay the vessel bare to a great extent, if the principal ligature is thrown off before the complete obliteration of the arterial tube, it is often impossible to apply a new one, on account of the small size of the incision and the depth at which the vessel is situated. If the opposite ligature be loosened and the artery tied be one of those which furnishes many anastomosing branches, as the carotid, the tibial, the cubital, and the radial, the retrograde course of the blood

soon causes a distressing hemorrhage, an accident which need not be feared if the artery was not cut between the ligatures. It also happens when wishing to separate them to as great a distance as possible, we apply the principal ligature very near to a branch of large caliber. All these difficulties have caused the abandonment of this method of Celsus and Ætius. It is entirely rejected in England, where it was formerly more in vogue than any where else.

It is well known, that a single ligature suffices to produce the prompt and solid obliteration of an artery, when all the circumstances concur to produce within and without it, the just degree of *adhesive* inflammation. Yet, occasionally, it has occurred in the human subject that the pressure exerted by the ligature has caused ulceration, and not adhesive inflammation; either because of the general weakness of the individual, or from a partial relaxation of the artery: it is ordinarily the third day after the operation that this accident has taken place, while the adhesive inflammation does not always pass through its stages in that time. The mutual union of the arterial walls may not take place, in consequence of too great a local irritation. The simple and unaided ligature, without any modification, does not prevent accidents, and especially the *consecutive* hemorrhage, because it is not the most advantageous means that can be employed to produce with certainty and promptness the solid obliteration of the artery.

We know that after applying several ligatures without success, and after various trials, Hunter employed but a single one: the greater number of practitioners imitated his example, but some added a second, called the ligature of reserve. Nevertheless, consecutive hemorrhage came on the seventh or eleventh day after the operation. This accident, which thus happened to me as well as to many other surgeons, determined me to adopt a mode which has since constantly succeeded:—it consists in placing between the artery and ligature a small cylinder of linen covered with cerate, which prevents, or at least very much retards ulceration of the arterial walls, which is produced by a single *circular* ligature.

The extreme confidence which has been placed in the sin-

gle ligature, appears to depend especially on the experiments performed by JONES. He proved that the first effects of a *circular* ligature applied to an artery, was a rupture of its internal and middle coats: DESAULT merely indicated this fact. Jones concludes that this rupture produces the obliteration of the vessel much more speedily and surely, although the impetus of the blood be supported by the external or cellular coat alone. Nature, says he, comes in to assist art; a clot of blood forms at first with rapidity in the arterial tube and presents a first resistance to the column of fluid: adhesive inflammation is afterwards developed in the three membranes of the artery, and gives place to an effusion of a *plastic* lymph between each of them, externally and internally, which contributes to the thickening of the point where the two membranes have been cut through, at the same time it causes the clot to adhere to the internal walls of the artery, and the latter with the adjoining parts. This process considerably accelerates the obliteration of the vessel.

I have already said that in repeating the same experiments by whose results Jones supports his opinion, that I was led to entirely opposite conclusions; I will now add here several reflexions: however great may be the resistance offered by the external or cellular coat, it can never equal that of all three coats together, as is proved by the hemorrhage coming on more promptly when the simple ligature is used. All anatomists know, that an extreme delicacy of the walls of the arterial system throughout occasionally occurs, and this disposition depends on the primitive organization of the individual. If in such a case we attempt to inject the vessels, they are ruptured at various points, whatever precautions are taken, and however slightly the injection be thrown in. We may conceive that this rupture of the vessels would take place more certainly in such a subject, if the internal and middle coats were divided by a ligature. But, as it is impossible during life to perceive the thickness of the arterial walls, we should without doubt prefer a ligature which is to be moderately tightened and does not rupture the coats at all. Besides, if

the adhesive inflammation is slow in appearing, the rupture of the membrane may cause the formation of an aneurismal tumour. At least this happens in inferior animals, but in man it is not so rapidly developed, and may happen when the ulceration of the cellular membrane ensues in consequence of the constriction, before the solid obliteration of the artery is effected, and this accident takes place much more rapidly than is generally believed. It often occurs in debilitated subjects, that the fibrinous clot does not acquire the degree of consistence necessary to the occlusion of the vessel, although the adhesive inflammation speedily supervenes; the same circumstance may happen in this case. It is evident that all these inconveniences are not to be feared when all the coats are preserved uninjured, and moreover it is demonstrated by experiments made on men and animals, that adhesive inflammation follows simple compression of the artery, as it does after the rupture of its coats. This mutual internal adhesion is effectuated in the same manner as in the serous membranes, to which class the internal membrane of the arteries belongs. A slight degree of pressure is sufficient for its production as the observations of DUBOIS, ASSALINI, and CRAMPTON, have proved. Besides, has it not often happened that an aneurismal tumour by keeping up a continued pressure on the ruptured artery, has caused its complete obliteration? Are there not numerous examples of a similar effect produced by tumors of different character? Before the superiority of the ligature was proved by experience, it is certain that the cure of external aneurism had been promptly and radically cured by compression. If this method was not always successful, it was solely owing to the difficulty of keeping the compressor in a fixed and continuous manner on the artery. Numerous examples exist of cures effected by this mode; such have been reported by S. FORMY, GUATANI, FLAJANI, BUZANI, and GARNERY.

There are strong reasons for believing that the arteries do not require a violent stimulus to excite them to inflame, and that in certain subjects probably more irritable, the arterial

system is disposed to inflammation, even from the influence of a moderate excitement. Why does not this phenomenon show itself as rapidly in the arterial tissue as in the cellular and muscular parts, since it is endowed with a very great degree of vitality, and receives an infinite number of vessels? HODGSON has seen inflammation of the internal membrane of an artery which was propagated along the inside of the vessel to the heart, in an individual who died subsequent to an amputation of his thigh. I have twice seen the same thing under similar circumstances. CLINE, ABERNETHY, and Professor MORIGI, have also remarked it, after the ligature of the femoral artery in a case of aneurism. HOEHME has observed inflammation of the hypogastric arteries in a child, in consequence of a ligature applied to the umbilical arteries. It is known that this accident is rapidly developed in the veins, and gives rise to symptoms analogous to those of typhus. The progress of inflammation is less speedy in the arteries, and it is very rarely propagated to the same extent. Commonly it does not extend more than an inch above and below the ligature. Experiments frequently repeated on animals, show that two hours after the application of a ligature, (without injuring the arterial coats,) that there is on the inside of the vessel a red line corresponding to the situation of the ligature, which indicates the commencement of the *adhesive* inflammation; twenty-four hours later, there is a red surface instead of the red line, an inch broad above and below the ligature, and of a more intense colour on the side next the heart. A multitude of little blood vessels may be distinguished. Throughout the extent of the inflammation, the membrane loses its transparency and polish; it becomes thicker, and resembles in appearance the inflamed palpebral conjunctiva.

The following experiments go to prove whether the *adhesive* inflammation and the obliteration of the artery take place more promptly in Hunter's operation, when the ligatures are drawn so as not to injure the internal membranes, or when they are ruptured.

*First Experiment.*—I tied the carotid in two sheep of equal

age and vigour. In one I used a flat ligature, and a small cylinder of linen covered with cerate, between it and the vessel. I tied the artery of the other with a simple string. They were killed at the end of four days. In the first sheep the carotid was covered for the space of an inch and a half around the ligature with a glutinous lymph. The ligature was readily removed after cutting it over the little cushion which served to prevent any injury. The external coat of the artery immediately under the cylinder far from being livid and contused, was sound, but thicker, and softer than in the natural condition. The carotid was opened longitudinally, and a conical clot of blood was found above and below the ligature, but strongest on the side next the heart; there was no sign of rupture. The clot nearest the heart was larger and longer than that on the opposite side; it was intimately united to the internal walls of the vessel by means of coagulable lymph effused in the cavity of the inflamed artery. The points of the surface of the membrane kept in immediate contact by the ligature were united by *vascular inosculation*.

In the second sheep the ligature was also covered by coagulable lymph. It could not be removed without much difficulty, because it was buried and concealed by the two ruptured coats of the artery. When the artery was opened, it was found to be inflamed above and below the ligature, but more intensely on the side next the heart. The rupture of the two membranes was manifest, and adhesion had only taken place between the opposite surfaces of the cellular coat. Between the broken and ragged edges of the internal membrane and the centre of the arterial tube, there was a species of membranous partition of a reddish, transparent, glutinous substance, formed by coagulable organizable lymph. Of the two clots, that which was nearest the heart was longest and most firmly attached by its base to the walls of the vessel. *The external membrane was evidently attacked by a commencement of ulceration, and had become so thin at some points, that the slightest effort would have served to rupture it, although but four days had passed since the operation.*

*Second Experiment.*—I tied the left carotid in two other

sheep, interposing the little cylinder. On the fourth day the ligature was removed from the first; that on the second sheep was left. Both were killed nine days after the operation. The artery was completely obliterated in the first, with all the appearances before enumerated. The result was similar in the second animal, except at the posterior part of the artery, where the ligature resting immediately against it, had produced a beginning of *ulceration*, which had destroyed the cellular coat. The two others had acquired a notable thickness.

*Third Experiment.*—A ligature was in the same manner applied in two other sheep: one was removed at the beginning of the third day, and the other at the end of the fourth. Both were killed on the ninth day. The mutual adherence of the internal walls of the artery was as solid in the one from which the ligature was removed on the third day, as in that suffered to remain till the fourth.

*Fourth Experiment.*—A similar ligature was applied to the carotid of a large and vigorous dog. It was removed on the fourth day, and the dog killed on the ninth. The same results were obtained, with the exception of finding the coagulable lymph effused on the inside of the artery proportionally more considerable than in the sheep, having obtained in the same lapse of time a greater degree of consistence. There was a very great thickening of all the coats which produced a gangliform swelling where they had been tied. The artery was obliterated to the extent of an inch below the ligature or on the side next the heart. It is probable that the obliteration of the vessel and the promptitude with which the coagulable lymph thickens and becomes organized, is in the direct ratio of the strength and vital energy of the animal.

*Fifth Experiment.*—The same ligature was applied in a robust and very active calf: it was removed on the fourth day, and the calf was killed on the fourteenth day after the removal of the ligature. There was a thick, irregular mass, formed by the hardened coagulable lymph around the tied

part: the coats of the artery were considerably thickened, and the obliteration of the artery was complete under the ligature. There was a fibrinous clot on the side next the heart, an inch and a half long, adhering by its base and by its whole surface to the internal membrane. Above the ligature was another pale red clot, partly absorbed.

*Sixth Experiment.*—The carotid of a horse was tied in the same manner; the ligature was removed on the fourth day, and the animal killed on the twentieth. The vessel was enveloped, and apparently pressed together by a compact, oval substance, an inch thick. Its cavity was filled by a thick, fibrinous, conical clot, of a yellowish colour, and adhering at all points. So intimately was it united at its base, that it was impossible to detach it from the walls of the artery, which for the space of an inch and a half, had begun to be changed into a solid body.

I have remarked in animals killed within three or four months after the application of this modification of the ligature, that the two fibrinous clots lose at first their red colour, diminish in volume, and eventually aid in changing the artery to a solid ligament.

Professor MISLEI, at the Veterinary School of Milan, has obtained analogous results by employing the ligature after my manner.

He tied in this way the carotid artery of a mare, so as completely to interrupt the course of the blood. Two hours after, the animal was killed. On opening the artery, the circular red line corresponding to the ligature was seen, without any trace of inflammation. In another horse the right carotid was tied in this manner, and the left secured with a simple cord: the animal was killed two hours after. The first ligature had only produced the circular line, without any lesion of the coats: the second had caused a ragged circular rupture of the internal coat, at the bottom of which the muscular coat was found to be redder than natural.

He observed the same phenomenon in a horse killed twenty-four hours after the application of the ligature. He saw in

all the cases the inflammatory redness extending itself an inch above and below the ligature, and always more intense on the side next the heart.

Professor Mislei wished to know whether this ligature would produce the same phenomenon in very old animals: he chose for this experiment two horses and a mule, enfeebled by advanced age and long-continued disease. In the first, the ligature was removed from the carotid thirty hours after its application; in the second, fifty, and in the third, forty-four hours after its application, the course of the blood having been intercepted in them all. In the second individual the adherence of the fibrinous clots was so strong, that it required the aid of the scalpel to separate them. In the mule, killed seven days after the ligature was applied, the artery was almost entirely changed to a ligamentous cord. The same results were obtained from the two horses in which the ligatures were removed forty-four hours after its application.

In short, other experiments he has just communicated to me, show that *ulceration* of the arterial coats, does not begin, when the ligature is applied according to my method, until the end of four days, if the ligature is left on, while it follows much more rapidly when it is made with a simple string, which is left in the wound.

From all these facts, it evidently results that the method of securing arteries I have indicated, has the triple advantage of promptly causing the proper degree of *adhesive* inflammation, of retarding to the greatest possible degree, the ulceration of the arterial walls, and of leaving all the coats of the vessel uninjured. The ligature should be made with waxed threads placed parallel to each other so as to form a little flat tape. Between this and the vessel we place a small roll of linen covered with cerate. The artery should not be denuded nor insulated to an extent greater than is necessary for the passage of the ligature. The length of the little cylinder should be such as to allow about the twelfth of an inch to project above and below the ligature, which ought to be about a line in breadth for the large artery of a limb. It should not be excessively tightened; but should be drawn so as to bring

the walls of the artery fairly in contact. It should never be placed near the origin of a large lateral branch. Such are the conditions requisite to insure a successful result.

The inconveniences which have been attributed to the cylinder of linen, have no existence, as is evidently proved by the experiments just detailed. On the contrary, it has the advantage of facilitating the division of the ligature, and of consequence its removal, which cannot be done without much difficulty and danger, if the blood still enters the artery after the third day, and if it is deeply buried in the circular groove resulting from its constriction. My experiments have also demonstrated that this method prevents the *ulceration*, or at least retards it, which is a very important point, since all three coats remain uninjured during the time the obliteration is going on. Another consequence of these experiments, and not the least worthy of consideration is, that it is not necessary to wait for the discharge of the ligature, since its prolonged presence may cause the ulceration of the artery; moreover, the internal adhesion of its walls and that of the two clots, suffice to arrest the course of the blood on the third or fourth day, at which period the ligature may be removed.

We may readily conceive that this last circumstance is an advantageous modification to Hunter's operation, since it averts the consecutive hemorrhage, and, (as we may immediately after unite the lips of the wound,) hastens the cure. I believe this method far preferable to that pointed out by LAWRENCE, because whatever analogy there may be between silk threads and other animal substances, whether their absorption does or does not take place, they must first produce the accident caused by the prolonged application of the simple ligature, that is the ulceration of the vessel.

The time when the ligature may be removed, is also that in which the dressings are commonly changed, except in cases where the patient is very feeble or very old. In such case it should be deferred until about the sixth day, for cicatrization does not commence in old subjects before that period. The simple *circular* ligature would almost produce perforation of the vessel in that time, as we have seen that it

causes ulceration of the external coat after the third day. In this case we should not hesitate to reunite the wound immediately, even should the throbbing be felt in the aneurismal tumour after the removal of the ligature, because in this way we always avoid the consecutive hemorrhage, or the necessity of amputating the limb. Moreover, however feeble the degree of inflammation produced by the ligature, it will cause a thickening of the coats of the vessel at this point, which retards the course of the blood and favours the formation of successive fibrinous layers, which soon fill the aneurismal sac and produce a *spontaneous* cure.

Although the great analogy existing between man and other animals would lead to the conclusion, that experiments performed on them would be analogous in result to those performed on man, it was necessary to confirm the fact by experiment: this is demonstrated by the three following observations, for which I am indebted to Professor PALLETTA, Surgeon in Chief to the great Hospital of Milan, as well as the fourth, which was obtained at the Hospital of Pavia.

The subject of the first observation was a man forty years old, with a popliteal aneurism which was first perceived in November, 1816. The tumour had acquired the size of a turkey's egg, when the ligature was applied on the eighth of January, 1817. On the fourth day afterwards it was removed, and the wound immediately united: the disease was perfectly cured, but the foot was lost from dry gangrene. This accident, fortunately very rare, is altogether independent of the method employed in the application of the ligature.

The subject of the second operation was a man sixty years old, who had aneurism in the bend of the arm, in consequence of a wound made in the artery by a bleeder. It increased rapidly in size, and the ligature was applied on the twenty-fourth of March, thirty days after it began. It was removed on the fourth day. The tumour was then considerably diminished in size, and the wound nearly cicatrized on the seventeenth of April. At the end of that month the cure was complete.

The third patient, (who was operated on by Dr. BIRAGHI,

assistant surgeon,) was fifty years of age, and of a sanguine temperament. He had popliteal aneurism of the right side, without known cause; the tumour was of the size of an egg. On the thirteenth of June, 1817, the femoral artery was tied, and *three days* after the ligature was removed. The tumour had already much diminished in volume; a month afterwards the patient was perfectly cured; nothing but a small hard tubercle existing in the ham.

In the fourth instance the ligature was applied near the superior half of the arm, for a wound of the left humeral artery. It was not tied below the wound. The ligature was removed at the beginning of the fourth day after the operation. The cure was completed at the end of five weeks.

Among the numerous examples of cure obtained by the use of this *temporary* ligature, the following are selected from the most recent, by Dr. C. P. OLLIVIER D'Angers.

Professor MAUNOIR, of Geneva, has communicated a case of ligature of the left common carotid for a supposed aneurismal tumour, situated at the angle of the lower jaw. The patient was a man 30 years old. The ligature was removed on the third day, and the obliteration of the artery was complete; the cicatrization promptly ensued. The tumour diminished speedily in volume, and the patient could sleep. Nevertheless it returned to its former size at the end of a month, with symptoms which caused a suspicion that its real nature had been misunderstood. But the complete obliteration of the carotid, was produced by the *temporary* ligature.

WATMANN, Professor of the Chirurgical Clinic at Inspruck, has cured in a month, by this process, a merchant 30 years old, affected with popliteal aneurism. The ligature was removed on the fourth day. The cure was complete.

Professor FRITZ, of the Chirurgical Clinic of Prague, performed a similar operation on a man 40 years old, and cured him within a month.

SAMUEL MEDORO, a surgeon of Padua, has equally succeeded in a case of popliteal aneurism. The patient, 43 years of age, was operated on April 16, 1821; the ligature

was removed on the 20th, in the morning, and the cure was complete at the end of November, the tumour having entirely disappeared.

A patient, 36 years old, was operated on for a similar aneurism, May 18, 1822, by Dr. JOLERO, a surgeon of Mantua. The ligature was removed on the fourth day; dry gangrene of the foot came on the 30th of the same month, advanced rapidly, and ceased after extending one-third up the limb. After the separation of the dead parts, the cure speedily ensued in the month of July.

### *On the removal of the Ligature.*

When a *temporary* ligature is applied on an artery, its removal after the third day may present some difficulties, and even be dangerous to the patient. At least the lips of the wound must be separated, and part of the organizable lymph which forms the first adhesions of the vessel with the surrounding parts be destroyed, which will retard the cicatrization. To obviate these inconveniences, professors PALLETA and ROBERTS propose to place one or two waxed threads immediately upon the artery, or rather between the little cylinder of linen and the ligature, before tying the vessel, and to make a running knot upon the principal knot. Aided by these threads the principal knot may be untied by drawing it towards the operator, after the running knot has been loosened. Professor UCCELLI places between the cylinder and the ligature a small piece of canulated metallic sound of the same length as the cylinder, and it is on this he cuts with the scissors when he removes the ligaturè on the third day after the operation. Professor GIUNTINI attached a simple waxed thread to one of the extremities of the small cylinder by which it is easily withdrawn at the same period without the necessity of introducing the fingers into the wound: an advantageous modification at this time adopted by prudent and skilful surgeons.

Notwithstanding these different changes, the fingers should always be introduced into the wound, whether to loosen the running knot, to relax the principal ligature, or to guide the

scissors or bistoury, which is often difficult to accomplish on account of the depth at which the vessel is placed, and which is often increased by the swelling of the surrounding parts, as has been observed, after taking up the carotid, axillary, and internal iliac arteries. We cannot then prevent the inconveniences mentioned, by these means, and I have attempted to arrive at this result by trying various processes. The following seems to fill all the indications in a satisfactory manner.

I take a common grooved staff, slit at its inferior extremity, to one of the edges of the groove two small flattened rings are fixed, having their greatest diameter proportioned to the breadth of the material used for the ligature. One of these rings is situated the sixth of an inch above the lower end of the canula, and the other a little below its *wings*. The bottom of the director is formed by a very narrow groove throughout its whole length. It terminates at a little more than half an inch from the inferior extremity of the director, which is slit thence to the end.

I use a small knife with a convex edge, whose blade is not more than five-twelfths of an inch long, and which is sufficiently thin to glide freely in the groove of the staff: at the base of this little knife there is an eminence which prevents the blade from advancing more than its cutting edge beyond the slit in the staff.

These two very simple and delicate instruments are sufficient for the division of the ligature, however deeply the artery may be tied. The end of the ligature hanging out of the wound is passed first through the lower, and then through the upper ring. By the aid of this sure guide, the staff is gently introduced to the bottom of the wound, until the lower ring is solidly applied on the knot of the ligature. Then the ligature is drawn slightly and secured around one of the wings of the staff, so as to prevent the lower ring from moving, or in any way separating from the knot with which it is in contact. The staff is carefully placed in such a manner as to have its groove in the same direction as that of the tied artery: to effect this it is sufficient that the

wings of the staff be directed transversely to the course of the vessel.

After this disposition has been effected, the operator holds the sound with one hand, while with the other he guides the little knife as vertically as possible in the groove. Arrived at the beginning of the slit, the blade sinks into it the more easily as its point is naturally directed by the little groove in which it was engaged. In continuing to press forward the instrument, the blade descends vertically and without varying between the two borders of the slit, and arrives at length at the ligature upon the knot, and transverse to its direction. It is sufficient then to support the blade lightly and give it a small sawing motion, to cut the ligature on the cylinder, which secures the artery from all lesion. The hand holding the grooved staff feels at the same instant the loosening of the ligature; as at this period, (the third day after the operation,) the ligature and cylindër are glued together by the coagulable lymph effused around the vessel, it is essential to assure oneself that the ligature has been completely cut before withdrawing the staff from the wound, in order to exert no pulling on the vessel, which however slight may have fatal consequences. We begin by first removing the little cylinder of linen by means of the thread attached to it, and then we withdraw the staff slowly and carefully, bringing away with it the divided ligature.

It may be perceived that I have mentioned but a single string left pendant from the wound, although commonly there are two ; but I have remarked that it was advantageous to cut one close to the knot during the operation, because it only increases the number of foreign bodies in the wound. It is not a matter of indifference to know which of the threads has been cut, when performing the operation I have recommended. For instance, suppose that we have tied the femoral artery of the right limb, and have cut the ligature which is nearest the inside of the thigh ; it is evident that the edge of the knife will encounter the remains of this end of the ligature before touching the proper ligature: even sup-

posing the operator to have two directors, one with the rings on the right and the other on the left, he will have equal necessity for knowing which of the two ends have been cut off. For all these reasons, I will add, that attention should be paid when we tie the artery, to make the knot in such a manner as to have it beyond the middle line of the cylinder on the side of the end we wish to cut it; hence the position of the ligature, which should be divided by the edge of the knife, will be found nearly on a horizontal plane, which will facilitate the division.

After the experiments I have made on large animals, in which the carotid is deeply seated, and still more after the third day from the operation, I feel assured that in employing the process I have indicated, the ligature may be removed with promptitude and facility, even when the vessel is seated at a depth of two inches without sensibly separating the lips of the wound, and without giving the vessel any injurious shock which can destroy the beginning of adhesion and retard the process of cicatrization. It is certain that the cases in which I have employed this method, the division of the soft parts has been followed by a much more rapid cure than when I was obliged to introduce the finger into the wound to effect the division of the ligature. In a large goat, whose carotid was tied after this method and the ligature removed by the instruments I have described, the wound was completely closed and cicatrized the fourth day after the removal of the ligature. I have no doubt but that analogous results would occur in man.

Professor Giuntini operated on the arm of a young woman in this manner, who died some months after of a pulmonic disease. The artery was found to be perfectly and solidly obliterated at the point where it was tied.

[The late distinguished Professor BECLARD, after experimenting on animals, came to the conclusion that the removal of the ligature does not prevent the ulceration of the vessel. When a ligature applied with sufficient tightness to intercept the course of blood, is removed before the obliteration is

complete, this fluid continues to pass as before, and the ulceration of the walls does not take place. If the ligature be removed after the artery is obliterated where it was applied, the ulceration which has begun continues to produce, and eventually effects, the complete division of the artery.]



ART. IV. *Remarks on the Male and Female Urethra* by M. AMUSSAT, *Anatomical Assistant to the Faculty of Medicine of Paris.* Translated by EDWARD JENNER COXE, M. D.

THE internal diameter of the urethra presents some differences of caliber in its four parts; the method generally adopted to prove this, is by the injection of wax, or some analogous substance; a process difficult to perform well, and generally distending the canal in one direction more than in another; in other respects, the results obtained by this mode are very variable, arising from the age of the subject, and from the state of the spongy tissue, which is more or less engorged with blood, according to the difference and duration of the disease, which caused the death of the individual upon whom such researches are made. After many trials of the above method, I have contrived the following, which consists in tying the urethra at its extremity, then to distend the bladder and its external canal moderately, by means of the ureters; if we then free this passage of the parts covering it, and reduce it almost to its mucous membrane, we see distinctly the form of the urethra; at its prostatic portion it is large and forms a cone whose base is behind; at its membranous portion it is slightly enlarged, and contracts itself opposite the bulb, to become suddenly widened at the commencement of the spongy portion, and diminishes insensibly to the orifice of the urethra. The first time that I made this preparation, I was much astonished in not finding any enlargement in the part corresponding to the gland, that is, in not meeting with any fossa navicularis; but I have repeated this prepara-

tion frequently enough, to assert that the usual state of the caliber of the urethra is such as has been described.

I believe that the appearance of an enlargement in the part corresponding to the gland, may be explained, by saying that the tissue of this part is less soft, its mucous membrane more adherent, and that in cutting the urethra, the two halves of the gland remain firm and well open, whilst the spongy tissue, properly so called, returns upon itself, and is lessened by the discharge of the blood which it contains; what proves that the fossa navicularis exists only in appearance, is that by stretching transversely the spongy portion situated behind the gland, we give it the same breadth as that found in this body. Besides it is sufficient to repeat the experiment pointed out, to be entirely convinced of what is here advanced.

The structure of the gland explains also why the urethra in this place is flattened from one side to the other, and the meatus directed vertically.

Internally examined, the mucous membrane of the urethra is first to be observed; in colour it is constantly whitish, and slightly rosy near the meatus only. Upon the inferior surface, and always internally, we find arranged along the median line, and from before backwards, small mucous lacunæ; their orifices present anteriorly; at the junction of the bulb and the membranous portion, that is at the narrowest part of the canal, the openings of the ducts from COWPER'S glands exist, which can only be perceived by pressing the small glands. Very near the bladder, and in the same direction, that is below, a small fungous eminence is seen, called *crista urethræ* or *verumontanum*; its extremity is pierced by two openings, which are the terminations of the ejaculatory canals. The summit of the *crista* is sometimes reflected upon itself, so that the mucous membrane forms around the two openings a species of valve or prepuce, in which it has been supposed that the catheter often becomes entangled; but it has a greater tendency to slide to either side of this small fungous eminence, than to stop immediately at its summit, as I have convinced myself by experiments made upon the dead body. Upon the sides of the *crista* are placed semicir-

cularly the openings of the canals from the prostate gland; behind we remark a transverse projection forming a distinct demarcation between the bladder and urethra; these two elevations, that is the crista and the transverse projection, unite in a right angle and divide the commencement of the canal upon the inferior coat into two lateral halves or small fossæ. It is there that bougies are often arrested, and not at the crista; still less frequently in the large mucous lacunæ, which by some have been supposed to exist in this place. Figure 1, describes the parts that I have pointed out.

When we pass from before backward the end of the index finger of the right hand upon the inferior coat, and press a little, whilst we hold the urethra stretched with the left hand, we feel on approaching the bulb, an enlargement corresponding to the commencement of the spongy portion; and still further on, the finger is stopped by a semicircular band, which is formed by the fibrous circumference of the commencement of the canal of the bulb. This band is the more distinct in proportion to the emptiness of the spongy tissue of blood. What proves that this obstacle is caused by the fibrous band of the bulb, and not by the perineal aponeurosis, as has been repeated on the authority of BELL, without any examination, is that we observe the same thing, and that the obstacle is the same, whether the urethra is in its natural situation, or entirely unconnected. When we carefully raise the mucous membrane covering the bulbous and spongy portions of the canal, we observe this membrane to be very thin and adherent, particularly to the gland; and that underneath it, there is another membranous layer, which prevents its coming in immediate contact with the spongy tissue. This layer is the fibrous membrane of the interior of the spongy tissue, which being continued with the external membrane, forms a fold all around the opening of the spongy tissue surrounding the urethra; it is this fold that forms the band of the bulb already spoken of; we can easily be assured of it after having removed the mucous membrane, by again passing the finger over this part thus stripped. (See figure 2.)

It is now easily conceived why the beak of the catheter so

often deviates from the natural course in this place, and consequently also why we there meet with the greatest number of false passages, and not in the membranous portion, as has been supposed.

In the membranous portion we meet with nothing remarkable; no obstacle exists in the natural state; when the prostate gland is diseased, a demarcation is felt between this body and the commencement of the membranous portion. If we pass the end of the finger over the part corresponding to the prostate gland, the crista is depressed, and the finger is arrested by a transverse band, which prevents our coming to the bladder. By cautiously elevating the mucous membrane covering these parts, we find in the crista the two ejaculatory canals, which pass between this membrane and the prostate gland; these canals elevate the mucous membrane and determine the form of the crista. Upon the sides of this small eminence, the mucous membrane is very adherent to the prostate gland on account of its secretory canals.

Further back, and constantly under the mucous membrane, we meet in the transverse projection or band above mentioned, with a well-circumscribed muscular layer; it is the sphincter of the bladder already mentioned, and which below is much more distinct than in any other part of the urethral circumference of the bladder. In front, it leaves below its level, a large semilunar excavation with the concavity anteriorly, and divided into two equal halves by the ejaculatory canals. Immediately below the sphincter is found the transverse portion of the prostate gland, which contributes in a great degree to elevate this muscular layer and render it prominent, even when the mucous membrane has not been removed. The transverse portion of the prostate gland, the sphincter of the bladder, and the mucous membrane, form then the band which arrested the finger. This band becomes very distinct, if we blow into the bladder through one of the ureters, and afterwards through the urethra; if it is all dried in this state, we discover in this part a real semilunar partition, a valvular fold in form of a crescent, very much resembling a portion of the dried pylorus. (See figure 2.)

The demarcation between the bladder and the urethra, is well defined only in the lower part, that is, at the place where a species of pyloric orifice of the bladder is found. The situation of this transverse projection, which is above the ejaculatory canals, and much behind their opening in the urethra, naturally offers an explanation much more satisfactory than has yet been given, in what manner the semen is evacuated whilst the urine is retained and vice versa. To be more explicit; the sphincter of the bladder, situated above the prostate gland closes the opening of the bladder while the ejaculatory canals remain free in the interior of this gland, which from the firmness of its tissue, prevents their being compressed. It appears to me that the ejaculatory canals by becoming elevated force behind and above, the fleshy fibres covering them, and form a band of these fibres more distinct there than elsewhere: for as I have remarked, the sphincter is not very distinct except in this place: moreover it is scarcely perceptible in infants; and in females the organization of the circumference of the urethral opening of the bladder is the same as in man, except that at the superior portion it is much thicker than the bladder, constituting a flattened sphincter and not a muscular band, as it is observed to be at the inferior part of the vesical orifice of the urethra in man.

The arrangement of the sphincter of the bladder and prostate gland at the inferior part, explains very adequately why the beak of the catheter is so often arrested and turned from its proper course in this place, without having recourse to the verumontanum and large mucous lacunæ, which do not exist in the healthy state of the canal. Frequently, even if the sound is retained, and the patient discharges a small quantity of blood, the crista is suspected to be in a fungous state; and it is particularly in old men that this happens. At this period of life, these parts are so soft and so easily torn that we might apply to them the term of *noli me tangere*, which is used to designate an entirely different state. I dwell upon this point because I am convinced that if these parts and the urethra generally were better known, catheterism would

become a much easier operation, and on that account much less dangerous.

If we now pass to the examination of the superior surface of the interior of the urethra, we find that it presents, like the preceding, the same colour and some mucous lacunæ situated along the median line; their orifices, also looking forward, are commonly more evident than those met with on the inferior surface. The superior surface differs chiefly from the other in not offering any depression; it is equal throughout the greater part of its extent. When we pass the extremity of the finger along this surface, it is not arrested until it reaches the part corresponding to the triangular ligament of the symphysis, but the finger passes easily on and arrives without difficulty into the bladder. The absence of an obstacle at the superior surface arises from the spongy tissue being so thin at the upper part that the passage from the spongy portion to the membranous is imperceptible. In short, the demarcation between the urethra and the bladder is so indistinct in this situation, that the finger passes with ease from one into the other.

The urethra is destined to the excretion of several fluids, which are the urine, the semen or spermatic fluid, that of Cowper's glands, and also of the mucous vesicles. For the excretion of the urine, the body is inclined forward, in order to relax the muscles of the abdomen and to diminish the curvature of the lumbar region. In the natural state when a man is standing up, the abdominal viscera are forced by the diaphragm against the posterior surface of the anterior parietes of the abdomen; this reacts and retains them between it and the vertebral column; in this state they press but little upon the pelvic viscera, and to be forced into this cavity the flexion of the trunk is indispensable. In tympanites the intestines cannot descend, notwithstanding the flexion of the trunk, in consequence of the gas which they contain, as well as the distention of the muscles; it is also observed that in this case the excretion of urine is impossible, notwithstanding the exertions made by the patients to discharge it; we are then obliged to use the catheter as has several times happen-

ed to myself. In the common state, the trunk being bent forward, the viscera are forced below by the combined action of the diaphragm and abdominal muscles. The intestines then slide along the posterior part of the anterior surface of the abdomen, upon the posterior surface of the bladder, descend into the pelvis, and gradually compress this organ from above downward, and from behind forward, as may be conceived by recollecting the situation of the peritoneum when it passes from the anterior surface of the abdomen to the bladder, and its form which is very different from that given to it when distended with air. The inferior portion of the bladder is likewise compressed by the rectum in males and the vagina in females; these organs themselves are supported by the levator ani muscle, so that the bladder is compressed superiorly, posteriorly, and inferiorly by soft organs, and anteriorly on the contrary, it is applied against resisting surfaces, viz. the symphysis and bones of the pubes; in the same direction and upon the sides, it is also supported by the internal obturator and levator ani muscles; it is also observed that when the bladder is entirely empty, it is found behind the symphysis and flattened from behind forward. Besides the external compression operating in all directions, the bladder possesses in its coats a muscular power independent of the will, which almost immediately compresses the fluid contained in it; this power resides in the very distinct muscular layer called detrusor urinæ. To be convinced of it, we need only examine the bladder of a living animal.

It is apparent that the bladder in order to discharge its urine, requires two powers, that is, an external compression by means of the surrounding parts, and a power inherent in its parietes. This is proved by knowing that if one of these powers is wanting, the excretion of urine cannot take place, as is seen in paralysis and tympanites of the bladder. It appears then that the sphincter of the bladder is only overcome by the action of the fleshy fibres of this organ. The longitudinal fibres act from the circumference of the urethral opening of the bladder to the periphery of the organ; the circular fibres also assist in dilating the bladder, contracting

themselves by a vermicular motion from the top towards the base. The alterations that the urethra undergoes during the discharge of the urine deserve much attention; at first the prostate gland is somewhat depressed, and consequently the commencement of the urethra, the thighs are separated, to allow the perinæum to be dilated, and give a greater space to the levator ani and perinæal muscles, and more particularly to those of the urethra; the penis is elevated, and sometimes elongated to straighten this canal; this is done instinctively when we are obliged to urinate. The urethra is passive in the discharge of urine until there is no more in the bladder to expel that remaining in the urethra, but then, in order to free itself of the remaining urine, the urethra contracts itself from the sphincter to the bulb, that is, the two lobes of the prostate gland approach each other by the action of the fibres surrounding them, and those of the membranous portion which follow continue to expel the urine; the urine then arrives in a part of the canal deprived of fleshy fibres, but the accelerator urinæ muscle is situated opposite, supplying their place, and contracting with sufficient force to expel the urine, not only from the portion of the canal to which it corresponds, but also from that situated anteriorly; sometimes, however, it happens, that a few drops of urine remain at the termination of this muscle, that is about the middle of the penis, and to discharge them it is sufficient to incline the penis downwards and give it a little motion. What has been remarked is noticed particularly in old men, because in them the accelerator urinæ muscle has lost its power; it is this muscle that suspends for a short time, when we wish it, the discharge of urine, or rather that by means of its contraction divides the stream into two halves by the flattening of the canal. That cannot, however, take place, until the abdominal compression has ceased to exist. As to the contraction of the bladder, it is not under the immediate influence of the will, since it cannot be produced but by external pressure, and consequently almost as soon as this ceases the other no longer acts. From what I have said it must not be concluded that it is the accelerator urinæ muscle alone that stops the dis-

charge of the urine, for this suspension may occur in females in whom this muscle does not exist. After the evacuation of the urine, we often experience, particularly in cold weather, a chilliness, sometimes even a kind of trembling, caused by the subtraction of the caloric that this fluid contained, and by the easier access of blood to this part.

The excretion of the other fluids through the urethra, generally occurs during the erection of the penis. In this state, which can be imitated on the dead body, by blowing into the spongy and cavernous tissues, the urethra undergoes very great changes; at first this canal becomes straight at least, in the perineal portion; opposite the bulb, the size of the urethra is very much diminished by the lengthening, and particularly by the pressure caused by this body, which cannot be swollen without encroaching upon the canal. In the spongy portion, the urethra is contracted almost to the same degree, because the spongy tissue being swollen, is thicker in that place where the canal is the broadest.

From the consideration of these data, we think that the swelling of the spongy tissue in the erection of the penis is of use in giving to the urethra a straight and firm direction, as well as in shortening and uniting the interior of this passage. The bulb stops the semen, that it may be discharged with force; without this obstacle, this fluid could not have been emitted to any distance, it would have been discharged in drops like the prostatic fluid before the complete erection of the penis.

The prostatic liquid appears to be destined to lubricate the mucous membrane of the canal, dried as it were, by the erection. Let us also take notice that the orifices of the ducts of Cowper's glands, are situated in this narrow part, and that their fluid appears to be appropriated to assist the passage of the semen. To express my idea by a comparison, I will say that this fluid is to the ejaculation what that of the tonsils is to deglutition; besides what other use could be assigned to the small glands of Cowper? The bulb in swelling, presses directly these small bodies, and the accelerator urinæ muscle contributes in a great degree to the compression. Let us

observe moreover, that the semen before being freely evacuated, is accumulated in the prostatic and membranous portions of the urethra, whose size remains the same during erection; this fluid is retained behind by the sphincter of the bladder, and in front by the bulb. In order to expel it, the levator ani, transversus perinæi, and even the sphincter ani muscles are convulsively contracted, assisted no doubt by the contraction of the parietes of the part of the canal containing the semen. By this double action, the semen clears the bulbous portion, and is projected outwardly by the spasmodic contraction of the accelerator urinæ muscle; so that the ejaculation comprises two very distinct periods, that in which the fluid is discharged by the vesiculæ seminales, and that in which it is discharged from the urethra. I think what is here advanced is very well explained by the structure of the vesiculæ seminales, which are not, as is generally supposed, small pouches, but really a continuation of the vasa deferentia, which are wound round upon themselves to form what is called the vesicles. These vesicles then are only two species of intestines, wound upon themselves, and whose circumvolutions are so intimately connected, that at the first view it is impossible not to take them for two small corrugated pouches. Their internal surface is strongly reticulated, and presents a fibrous structure similar to that of the auricles of the heart; that is to say, I take them to be fleshy fibres, and that these small intestines ought to act in the same manner as the large ones. It has not been my intention to describe in this place the vesicles, I propose doing that in a separate publication. From the true structure of the vesicles, the first period of the ejaculation can be very well admitted: and if the contraction of these canals was not indispensable, what would prevent the semen from passing out during the excretion of the urine, and more particularly in that of the fæces? This is I believe what we observe in weak persons.

Surgically considered, and particularly as regards catheterism, the urethra is a flexible canal in its anterior three-fourths, this disposition enables us to give it three principal directions; in the first place, by elevating the penis upon the

abdomen, we cause the urethra to describe a great curvature whose concavity presents superiorly; the middle of the curvature corresponds then to the front part of the inferior portion of the symphysis. By lowering the penis, the canal describes in its anterior half a curvature in an inverse direction from the preceding one, or rather an angle open below, and whose summit corresponds to the place where the penis is corrugated in a state of relaxation, that is opposite its suspensory ligament. Lastly, if we hold the penis in an intermediate position, the urethra is then almost straight, so that we can sound this passage with an instrument that will assume one of the three directions that I have pointed out. But which is the best to be employed in the healthy state of the canal? I will endeavour to demonstrate this in another essay; I will only observe in the meantime that whatever instrument we employ, we should always give the instrument we intend using the form of the urethra. We should be particularly careful to pass the point of the catheter along the superior surface, which is to be taken as a guide to the middle of the symphysis, and must not be abandoned, since, as I have shown, there exist at the inferior surface two obstacles; one at the fibrous pouch of the bulb, and the other at the prostate gland, before the sphincter in what may be called the pyloric orifice of the bladder.

Besides the difficulties that catheterism presents in common cases, there is another met with known by the name of spasm of the urethra. Much has been said, although very vaguely, of the spasm that the urethra often experiences when we sound this canal; I will endeavour to show what are the true causes of this spasmodic contraction. When a bougie is introduced into this passage, and is not pushed forward, it is generally forced out, and as much more quickly as the canal is less accustomed to the presence of this body. It is principally by the action of the accelerator urinæ muscle that this effect is produced, as it compresses forcibly from behind forward on account of the obliquity of its fibres; the proof of this is, that the effect is less when the sound has advanced further forward, because the muscle has not as great a hold

upon the instrument, and the prostatic and membranous portions, although possessing a muscular layer, only act by compressing the sound circularly by a vermicular motion; this action of the two first portions of the urethra, and especially that of the sphincter of the bladder, deserves the greatest attention; for, as regards the beak of the sound, they act in the same manner as the sphincter ani does upon the finger when we endeavour to introduce it into the rectum; if the finger is not introduced suddenly, the sphincter resists, if, on the contrary, it is forced immediately on before the contraction is excited, it enters with very little difficulty. It is the same with the sphincter of the bladder and prostatic and membranous portions. This is the reason why the catheter is so often stopped in these parts, and also why it is so difficult to overcome this contraction; when it has been once produced, whether we lower or elevate the point of the catheter, it forces along with it that portion of the urethra where it was confined, without in the least altering its direction, for anteriorly the calibre of the urethra is almost entirely obliterated. We frequently see a skilful practitioner unable to sound a patient, that one unaccustomed to catheterism will a short time after sound with ease. This can only be explained by the spasmodic contraction which was very strong in one case, and in the other was less, or taken by surprise, (if I may be allowed the expression.) In like manner we should not attempt to overcome this contraction, if it exists, but endeavour to penetrate into the bladder by introducing the instrument a second time, or by dilating this part of the canal.

After what has been said relative to spasm of the urethra, we see that this species of contraction can only occur in the posterior two-thirds of the passage, since the anterior third does not possess any fleshy fibres, although they have been mentioned by some writers. The spasm of the middle third produced by the accelerator urinæ muscle is the strongest, but is easily overcome, since it does not act circularly; the spasm of the other third, and particularly of the sphincter, is commonly, I think, one of the greatest obstacles to the introduction of bougies into the bladder. All that I have ad-

vanced in this place upon what is generally known by the name of spasm of the urethra, is the result of experiments made upon living animals. I will add, that after having laid bare the urethra of a dog, if the membranous portion is pressed between the fingers, we feel that it is firm, and that its coats are very thick; as soon as a sound is introduced, we observe the phenomena that I have described; if the sound is withdrawn immediately after, the urethra is felt to contract quickly once or twice with considerable force between the fingers; if this canal is forcibly distended with air, the part mentioned dilates a little, but as soon as the effort ceases, the air is suddenly forced into a more dilatable portion of the urethra.

I have made an observation upon the bladder of living animals which ought to be noticed in this place; when a sound has penetrated into its cavity, if the beak touches lightly its parietes, the corresponding fleshy fibres appear to separate, and the extremity of the sound is seen through them so distinctly, that it might be said to have pierced its coats, which fatal accident the slightest effort might produce.

Another experiment upon living animals shows that we have been deceived in supposing that the levator ani muscles could supply the place of the sphincter of the bladder. This experiment consists in removing the symphysis of the pubis, and destroying that portion of the muscles, to which this action had been ascribed; we see then that the urine is retained in the bladder as well as before, and if in this state a catheter is introduced as far as the sphincter, the urine will not be discharged until we have passed it into the bladder.

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### EXPLANATION OF THE PLATE.

Figure 1, represents the pelvis of an adult seen from the anterior to the posterior parts; the symphysis of the pubis has been removed to allow the interior of the bladder and urethra to be seen in their natural situation, and cut open from above.



ff. The muscular portion uniting the two lobes of the prostate gland.

GG. The bulb.

HH. The spongy tissue of the urethra.

II. The gland.

J. The internal fibrous membrane of the spongy tissue of the urethra deprived of its mucous membrane.

K. The pouch of the bulb formed by the fold of the fibrous membrane, going from the exterior to the interior.

L. The distinct orifices of the ejaculator canals in which are two pins.

MM. A portion of the superior surface of the prostate gland, exposed by the removal of the mucous membrane.

NNN. The sphincter of the bladder, situated above and a little behind the prostate gland.

OO. The orifices of the secretory canals of the prostate gland. (They are not visible in the plate.)

P. The lower fundus\* of the bladder deprived of the mucous membrane.



ART. V. *On Periodicity and Lunar Influence in Diseases.* By  
JOHN BELL, M. D.

I SHALL endeavour on this occasion to adhere to my first intention, of being the historian of all those changes, whether physiological or pathological, which take place in the animal œconomy at regularly recurring epochs. The facts first laid down clearly and circumstantially, we may then commence the work of deduction, and from effects ascend to causes. On a former occasion I detailed the series of actions in the living body during the several periods of the diurnal cycle. Extending my view at this time, I hope to sketch those vital phenomena either only evidenced, or greatly exalted in their display at hebdomadal and monthly intervals; and those

\* Called "Bas fond" by the French.

others again which are evidenced at particular seasons of the year. As in the first case the time of the recurrence of diseases was measured by the hours in the period of the revolution of the earth on its axis—in this second part of our subject the measure to regulate us in our inquiries is the days and weeks, or portions of lunar periods of months, in which the annual revolution is completed, and the earth and its dependencies subjected to the action of both the sun and moon whatever that action may be. While this view of the subject is favourable to methodical arrangement, it does not pledge us to the admission or adoption of any peculiar cause or series of causes for the phenomena in health and disease, to which attention is now requested. The philosophical caution of SYDENHAM, who, by the way, is seldom backward in offering explanations of every thing he writes on, ought to be more generally imitated. “I would fain know, for instance,” says this accurate observer, who had just before disclaimed any pretensions to account for the difference in intermittent fevers, “why a horse comes to his full growth in seven years, a man in twenty-one; why some plants usually flower in May, and others in June; not to mention innumerable other effects whose causes are hard to be discovered. Now if the learned ingeniously confess their ignorance in these points, I see no reason why I should be censured for being silent in a matter not less obscure, and perhaps wholly inscrutable; especially as I am persuaded that nature proceeds in this case, as in all others, with a certain regularity and uniformity; the matter of tertians and quartans not being less subject to and governed by the laws of nature than all other kinds of bodies are.” Sect. I. Chap. V.

Extending this idea, we may lay down as a law of the animal œconomy, the intermission of action or functional movement, both in health and in all diseases. The ingenious BICHAT endeavoured to draw a line of distinction between the functions of nutrition and those of relation, in the continued action of the former, and intermission of the latter; but though the sleep of the organs of relation, (brain, senses, and voluntary muscles,) alternate regularly with their waking and

active state, we are not therefore to deny the equally well ascertained fact of alternate depression and excitation in the organs of nutrition, (heart, lungs, and digestive canal, &c.) Again then it may be affirmed, that the feature common to all diseases, not even excepting the continued fevers of the nosologists, is the intermission or diminished intensity of morbid action during a certain determinate period, and following the occurrence of exacerbations or paroxysms. This position, supported by the tenor of my former communication, need not now be sustained in all its latitude, but rather restricted to a class still numerous of diseases, which make their invasion after regular periods, and pass away after a certain time, leaving the person thus attacked in the comparatively free use of all his functions, and competent to the discharge of his usual every day business.

Of these diseases the first in importance, if we regard their prevalence, are the fevers to which the term intermitting has been emphatically applied. They have been divided into three varieties, *quotidian*, *tertian*, and *quartan*—the first commencing and terminating every day, the second on alternate days, and the last on every fourth day. The alleged peculiarities of the quotidian making its attack in the morning, a tertian at noon, and quartans in the afternoon, though supported by the testimony of CLEGHORN, BALFOUR, CULLEN, and others, are not of such unvarying occurrence as to require their being received as a law. During the last few days I attended two young men, brothers, in intermitting fever, the one with quotidian, in whom the paroxysm came on at an hour after mid-day; the other with tertian, in whom the fit came on early on alternate mornings. However, this point may be settled, we ought not to forget an extremely important remark of Cleghorn, as illustrative of the well defined persistence of these fevers in their periodical recurrence, notwithstanding the anomalous symptoms of the successive paroxysms—"and the longer I was conversant among the sick," says this physician, "the more I was surprized at the constancy of nature, in the production and progress of tertian fevers; their periods being perfectly similar in the Spaniards

and in the English; and sometimes not very different in him who lies upon the bare ground, destitute of assistance, and those who are treated in the most judicious methods, under every advantage of fortune, and frequently neither the patient's intemperance, nor unskilful management, can alter their stated course and prevent their terminating in recovery. So much are those mistaken who imagine that the bent of nature in acute diseases can be altered or controlled by every trifling accident or insignificant prescription.\*

Connected with this view of the preservation of periodicity in tertian fevers, is the circumstance of paroxysms commencing much more frequently from eight o'clock in the morning to eight in the evening, than from evening to the morning. Whatever may be the deviations in time from the precise epoch of the return of intermitting fevers, as in tertians, which often have a latitude of two hours sooner or later, (that is from forty-six to fifty hours,) and the consequently different hours when they make their attack, there is still evidenced a strong indisposition in the fever to come on between eight at night and six in the morning.† Nor can we be unmindful of the opinions advanced by Sydenham, that vernal intermittents might be wholly left to themselves, and by HUXHAM that they are salutary; and the remarks of FORDYCE that regular tertians, the paroxysms of which are terminated in less than twelve hours, (returning from forty-six to fifty hours,) are very rarely fatal in temperate or cold climates. It has hence been inferred by many practitioners that the disease should be allowed to go through its natural course rather than risk the application of any remedy which may have a tendency to cure it, lest the disease should be disturbed and its salutary effects be prevented. Could we suppose intermitting fevers to consist merely in an uniform succession of associated movements, and not dependent for their cause on changes in the fluids or organic lesion, but kept up by external, (atmospheric or

\* Observations on the Epidemical Diseases of Minorca.

† Fordyce on Fevers.

planetary,) agencies, we would the more readily admit the correctness of the above opinion, and arrive at something like an explanation of periodicity in diseases. Let us not, however, anticipate conclusions, but proceed to note the gradual subsidence of intermitting fevers as the year advances either from the vernal or autumnal equinox, and their almost entire disappearance as we approach the solstitial epochs of summer and winter. The quotidian variety is infinitely more common in the spring than in the autumn, while the quartan is almost exclusively an autumnal disease. About the time of the equinox these fevers assume a surprising variety of forms, and very often counterfeit continued fever, having long redoubled paroxysms. As regards the fatal termination of the disease not only says CLEGHORN, is there "a possibility in many cases of foretelling the day, but likewise the hour on which the patient will expire, for that stage of the paroxysm which he usually got over with most difficulty, will most probably in the end prove fatal, as GALEN has also remarked."

The entire period of the duration of the respective varieties of intermitting fever cannot be stated with any degree of precision. We are told indeed on very good authority,\* that if a quotidian be perfectly regular, it leaves the patient in *about ten weeks*, a tertian seldom in less than *fourteen weeks*, while a quartan is apt to run on for *five months* before there is any sign of abatement, and it is usually *six, seven or eight months* before the disease entirely ceases. This calculation of course applies to those fevers which are left to run their course without the administration of any remedial means.

As substitutes for periodical febrile attacks, or occasionally the representatives of these latter, after they have lasted a certain time, it is not uncommon to meet with ophthalmia, hemicrania, pain in the stomach, pleuritis, cholera, convulsions, and even coma recurring at stated intervals. Sometimes

\* Fordyce.

these maladies come on without any perceptible change in the pulse, but accompanied by the most acute pain.

Of the diseases which obey weekly periods, continued fevers are mentioned by many authors. They have daily exacerbations for the first seven days, at which time they attain their acmè: during the second week they remain nearly stationary or have exacerbations on alternate days, and in the third week decline by exhibiting paroxysms of diminished violence on every third day. Usually, in warm climates, a partial solution of the disease takes place on the seventh day, sometimes a complete crisis, though more commonly the fourteenth is the critical day. On the fifteenth, the disease begins to abate, and during the third week there is a gradual disappearance of all the morbid symptoms.

It is admitted by those who have patiently watched the progress of fevers, that, in the ordinary course of regular continued ones, the patient may recover without the aid of medicines, either by a crisis which terminates the disease, most commonly in the second or third week, in less than twenty-four hours; or by the disease beginning to diminish after the fourteenth day, and going off itself, without any marked crisis.\*

The variolous fever in natural small pox, commences on the fourteenth day, inoculated on the seventh. In the distinct kind, the fever and eruption occupy the first week or quarter of a lunation—while maturation ending in desiccation, takes up another.

Epilepsy has been found in some cases to recur every fortnight—in others every three weeks.† The monthly return of this malady is of very common occurrence. In the instance of a female mentioned by PINEL, it came on every month, till she had attained her thirty-eighth year, when there was a cessation of the menstrual discharge, and disappearance of epilepsy. Whilst in another case, given by the same author, the disease appeared regularly every month after the menstrual secretions had entirely ceased.

\* Fordyce.

† Pinel, *Nosographie Medicale*.

Hæmorrhoidal flux frequently comes on at monthly intervals.\*

Hæmoptysis attacks in a great many instances at the expiration of every month, and similar recurrences are told of all hæmorrhagies.

Fluor albus is occasionally found to make its appearance, with the same regularity, and at the same intervals as the menses.

Asthma, erysipelas, and gout, have been known to exhibit their monthly paroxysms. Occasionally, certain diseases appear semi-annually, and in a more particular manner about the time of the equinoxes; such as the hæmorrhoids, apoplexy, epilepsy, maniacal paroxysms. Some cutaneous diseases are renewed regularly in the beginning of spring and middle of autumn.†

The most superficial observer of nature, cannot but be sensible of the potent influence of the seasons on man, in health as in disease: though the most accurate investigator into the causes of things, cannot satisfy himself as to the successive changes in the animal œconomy being brought about by the differences in the sensible properties of the atmosphere, or elemental peace or strife. The genial warmth of spring will hardly account for the strong excitation of the nervous system generally, and of the cerebral organs in particular, during that season,‡ when also hæmoptysis, pneumonia, pleurisy, rheumatism, and gout, make their attacks, and when anginose affections, catarrhs, herpetic, and leprous disorders, and pustules are so frequent.

Summer again brings in its train cholera, diarrhœa, gastric and bilious fevers, colics, vertigo and delirium. The greater number of insane persons become so in summer, and the dis-

\* Pinel, Darwin, Meade, Moseley, &c.

† Alibert. *Maladies de la peau*.

‡ The aphorism of Hippocrates, that in this period of the year, insanity and complaints from *atra bilis* arise, is confirmed by Esquirol, who says that on the approach of spring there is great agitation among the maniacs.

case is exasperated at this season. June, July, and August, give at Paris the greater number of maniacs.

Autumn, "fading into age" with a temperature as elevated as that of spring, and vicissitudes not more abrupt, imprints on all nature a very different stamp. Maturity is fast followed by decay, and vitality is summoned to a display of all its energies to resist the tendency of organized bodies to dissolution and putrefaction. Every housekeeper knows the extreme readiness with which meat will putrefy in the autumnal months. Congestions, irregular local determinations, and organic alterations, are at this time opposed to the skill of the physician. The brain, liver, and intestinal canal all suffer, and the animal œconomy in this more than any other season loses the habitual rhythm of its functions.

Winter, sullen and sad with all his rising train of vapours, clouds, and storms, bids animated nature rest. The vegetable kingdom is thrown into a profound sleep; and in the animal, part are cold and motionless as the rocky caves and fissures where they rest, and part struggle against the depressing power, and barely live the weary round. Man, the lord of all, though "immersed in furs," or flying to artificial heat, is still constrained to acknowledge his sensibility diminished, and vitality threatened. There would seem to be both *engourdissement* and *engorgement*. The larger vessels are distended without uneasiness being announced to the centre of perception.

In this season people suffer from pains in the loins, side, and breast, catarrhs, hoarseness, pains of the head, vertigo, apoplexy. Winter is particularly unfavourable to old persons, who can ill bear the stupefying influence of cold. It is at this time also that contagious fevers rage with most violence.

The solstices are usually marked by increased disease and mortality, particularly the summer one. The general opinion of this season on mental aberration is indicated by the great poet, in his comedy of Twelfth Night, where he makes Olivia exclaim of the conceited Malvolio, "why this is very

midsummer madness." The equinoxial epochs are also dangerous, especially the autumnal one. Hæmorrhoids and attacks of apoplexy and epilepsy are then very frequent, and the mortality greater than at other times.

Having now presented a brief outline of diseases recurring at stated intervals and assuming a periodical character, I may next, with more hesitation, however, connect this periodicity with certain planetary changes, and if I do not succeed in pointing out cause and effect, I shall, to say the least, have exhibited some curious coincidences.

No person, learned or unlearned, can be ignorant of the alleged influence of the moon on vegetation and animal life. In support of this opinion we hear cited the belief of an entire people, facts without number from the vulgar, and analogical argument from the philosopher.

The explanations of this planet's power over the animal œconomy are threefold, 1st, in reference to her attractive force; 2nd, her light; 3rd, the plays of electricity to which she gives rise.

The known operation of the moon on the waters of the ocean in producing, in conjunction with the sun, their rise and fall, and the phenomena of tides, render it pretty certain that the so much lighter atmosphere must also have its tides. Accordingly, we find D'ALEMBERT and TOALDO, regarding the attraction of the sun and moon on the air as the principal cause of the wind. It has been, moreover, ascertained by a journal kept for forty-eight years, that the mean height of the barometer was greatest when the moon was in her apogee, or farthest from the earth, and least when in her perigee, or nearest our globe. Of the flux and reflux in the atmosphere, as evidenced by the barometer twice in twenty-four hours, notice has been already taken. These alternate movements are independent of the seasons, or of variations of temperature, and take place on the highest mountains. We learn, moreover, that these barometrical tides undergo certain variations analogous to those of the waters of the ocean at the equinoxial or solstitial points. Whatever, in fine, may be the modifications in the state of the atmosphere, by the dilat-

ing effects of heat and condensing ones of cold, or its various electric changes, or the quantities of watery vapour it contains, we cannot fail still to refer much to planetary attractions, when we look upon the grand atmospherical revolutions, the impetuous winds of the equinox, and the various monsoons in the eastern hemisphere.

Whether the moon exert an attractive power on the fluids of living organized bodies, vegetable and animal, is a question of much more difficult solution, but to the examination of which we shall soon approach.

The second property of the moon by which her influence is attempted to be accounted for, is her light.

The thermometer is not altered by exposure to the moonlight, which, however, has, we are told, an evaporating power. TOURTELLE asserts, that of two vessels of equal capacity and form, and containing equal quantities of water, exposed for several nights out of doors to the moon, the one which has the rays to it intercepted by an umbrella will loose less by two lines and a sixth in nine nights, than the other which was exposed to the direct rays. This evaporating power of the moon led ST. PIERRE to the opinion of its light melting ice.\* He grounds his belief in part on the equivocal expression of Pliny, "*glaciem refundat*," and erroneously speaks of the heat of that luminary.

That exposure of animal matter to the moon's beams accelerates its putrefaction, is, we may presume, an indisputable fact, attested as well by sailors and fishermen, as by butchers and housekeepers generally. Sound fish is rendered soft by the moonlight in a few hours, and fishermen are careful to cover from its rays the fish they have caught. Attempts have been made to explain this effect by the greater formation of dew and precipitation of moisture on articles thus exposed.†

The third and last mode of lunar action, has been by some referred to the changes in the electric fluid produced by the moon. As arguments in support of this opinion, we are told

\* Studies of Nature, vol. 1. p. 36. Philad. edit.

† Wells on Dew.

of the general occurrence of earthquakes and volcanoes at the times of the moon's perigee or apogee, conjunction or opposition. These phenomena, as well as hurricanes, which also come on at the above epochs, are usually admitted to be the direct effects of electricity.

The reader can now of himself bring these premises to bear upon the main question, and when he has acknowledged the potent influence of the moon, first on the solid parts of our globe, as in earthquakes and volcanic eruptions; secondly, on the waters which partially cover it, as in tides; and finally on the atmosphere surrounding it, as in hurricanes, equinoxial winds, and barometrical tides, he will have the less backwardness in inquiring into her operation on living bodies. In the very changes above alluded to we find a play of agencies adequate to produce all the alleged effects of lunar influence; but it may be asked, shall we confine our views to these secondary causes, light, electricity, and atmospherical vicissitudes, when we can show that these are themselves dependent on the moon. In other words, ought we not to prefer the one simple cause to the many complex ones, when by the first we can explain all the phenomena, to solve which the combined aid of the second are barely competent.

Here we may avail ourselves of the ideas of the celebrated LA PLACE, as conveyed in the following sentences of his *Essai Philosophique sur les probabilités*. "The singular phenomena resulting from the extreme sensibility of the nerves in some persons, have given birth to various opinions respecting the existence of a new agent, called animal magnetism, the action of common magnetism, and the influence of the sun and moon on certain nervous affections; finally, respecting the impressions which may be produced by the proximity of metals or running water. It is natural to believe that these causes exert a feeble action, and that it may be altered by accidental circumstances. But, because in some cases it is not evidenced, we are not therefore to deny its existence. We are so far from understanding all the agents of nature and their divers modes of action, that it would evince little philosophy to deny phenomena, solely

because they are inexplicable in the existing state of our knowledge, though we ought to examine them with an attention the more scrupulous, as the difficulty of admitting them seems greater; and it is here that the calculation of probabilities is indispensable to determine to what extent we must multiply observations or experiments, in order to obtain in favour of the agents, which they point out, a probability superior to the reasons which we may otherwise have for not admitting them."

The power exerted by the moon over vegetation, was recognized by most of the ancient writers on agriculture and natural history, from ARISTOTLE down to CATO the elder, VARRO, PLINY, COLUMELLA, MACROBIUS, and PALLADIUS. Experiments made in Tuscany by order of the Grand Duke FERDINAND II. to determine what effect was produced on the wood, by cutting down trees in different quarters of the moon, did not confirm this opinion. In the intertropical regions, however, as in the West India Islands for example, the belief of lunar influence on vegetation is general, and so firmly rooted, that for wood cut down for the purposes of building, in the first quarters of the moon, payment cannot be exacted if the purchaser chooses to make an appeal. The same reason is assigned there which Macrobius advances, viz. that wood swelled with sap during the moon's increase, is thereby unfit for the purposes of carpentry.\* Hence, that is not so forced a comparison between the moon in her increase and decrease, and the seasons of the year. "The time from the new moon to the first quarter, represents the spring; from thence to the full, the summer; the third quarter, the autumn; and the last quarter, the winter."

An effect attributed almost universally by mariners and the inhabitants of eastern climes to sleeping with the face expos-

\* I have been informed by a very intelligent gentleman from Porto Rico, that if a tree called *Guarawao* be incised in the moon's increase, sap flows freely; but on the decrease of that planet, none flows; and that if it be cut down in the former period, the wood readily rots. There is also a kind of cane in the island which abounds in juice in high tides but is dry in low ones.

ed to the moon beams, viz. swelling of the eyes and face, and partial blindness has never been disproved, nor otherwise satisfactorily accounted for. In Upper Egypt and Arabia, there is, however, a correspondence between the diseases of the eyes and skin, and the phases of the moon: the former increasing and decreasing with the latter. “PLINY mentions the same phenomenon as to the ophthalmias in cattle; corresponding with what the writers on the diseases of horses, called moon blindness, or lunatic eyes.” Herpetic eruptions are very common in Cayenne, and are influenced by the changes in the moon, drying up and disappearing almost completely on the wane of that planet, but returning with increased force at the new and full.\*

The ulcerated legs of negroes have been seen to ooze with blood, when they have been sleeping or long exposed in the moon’s light: and negro women with sore legs, in the West Indies, when the menses are obstructed, have monthly discharges of blood for several days, from the ulcer, at the full moon.† MEADE, in his *Essay on the Influence of the Sun and Moon upon Human Bodies*, mentions the case of a young man, who, after impure coition, had an ulcer on the glans penis, which ran with fetid matter at every new moon, and after some days stopped. This continued for some months, “till he was put into a proper course of medicine, whereby he was perfectly cured. BAGLIVI, (De Exp. circa Sanguinem Op. Edit. Venet. p. 230,) speaks of his knowing a young man who had a fistulous opening from the colon near the liver, from which copious stercoraceous and mucous discharges took place every full moon; and diminished so regularly and uniformly in its wane, that the patient was able to determine thereby the quadratures of that planet.

But of all diseases, that which more emphatically justifies the appellative of lunatic, epilepsy stands first in the experience both of ancient and modern physicians. On this ac-

\* Bajan, *Maladies de Cayenne*.

† Moseley on *Tropical Diseases*, &c.

count those affected with the disease, were by the Greeks called Σεληνιακοί, and by the Latins, *Lunatici*. The assertions of Galen that the moon governs epileptic cases, is amply confirmed by PITCAIRNE, MEADE, and MOSELEY, each of whom gives many instances of the regular recurrence of the disease at the new and full moon. The two latter named writers were able to predict the times of the fits, and Moseley assures us that in young persons, they are easily prevented by adopting suitable measures three days previous to the new and full moon; and he very justly remarks that a knowledge of this kind might save many lives and great distress, from the afflicted with epilepsy falling down in the streets and public places, and from precipices and buildings. Those readers who cannot have access to Meade's Essay, will, I am sure, be gratified with the instructive details of a case of epileptic disease in his own words. "But no greater consent in such cases, was perhaps ever observed, than what I saw many years since in a child about five years old; in which the convulsions were so strong and frequent, that life was almost despaired of, and by evacuations and other medicines very difficultly saved. The girl, who was of a lusty, full habit of body, continued well for a few days; but was at full moon again seized with a most violent fit. After which, the disease kept its periods constant and regular with the tides. She lay always speechless during the whole time of the flood, and recovered upon the ebb. The father who lived by the Thames side, and did business upon the river, observed these returns to be so punctual, that not only coming home he knew how the child was before he saw it, but in the night has risen to his employ, being warned by her cries, when coming out of the fit, of the turning of the water. This continued fourteen days, that is, to the next great change of the moon, and then a dry scab on the crown of the head, (the effect of an epispastic plaster, with which I had covered the whole occiput in the beginning of the illness,) broke; and from the sore, though there had been no sensible discharge this way for above a fortnight, ran a considerable quantity of limpid serum. Upon which the fits returning no more, I took great

care to promote this new evacuation by proper applications, with desired success for some time; and when it ceased, besides three or four purges with *mercurius dulcis*, &c. directed to be taken about the new and full of the moon, I ordered an issue in the neck, which being thought troublesome, was made in the arm. The patient, however, grew up to woman's estate, without ever after feeling any attacks of those frightful symptoms."\*

Chorea or St. Vitus's dance is likewise found on many occasions to be obedient to lunar changes. The same subservience is found in vertigoes and apoplexies. Wounds of the head are most dangerous at the full moon.† "Apoplexies and palsies," says the learned MOSELEY, "are very common among aged people; and in the course of fifteen years at Chelsea Hospital, I have rarely known an instance of either of these disorders happening at any other time than at the change, or at the full of the moon." This is understood of these maladies when they prove fatal, or cause a lasting destruction of the power and motion of parts. It is not correct to call those disorders paralytic which have their monthly returns as Meade, quoting PRISO and TULPIUS, has done. We ought rather to class such with hysteria, which is often periodical in its attack. The following precautionary observations are too apposite to our subject to be omitted. "Short-necked, comatose, plethoric, gross people, where any mischief is latent, are always affected near the new and full moon. Such habits should be carefully watched a few days previous to these periods; and if any symptom indicative of apoplexy be observed, how easily is the storm prevented, by bleeding, cupping, purging, revulsion, abstinence, or such prompt measures as the case may require."‡

The correspondence of the attacks of asthma with lunar epochs remarked by VAN HELMONT, has been fully con-

\* A vast number of similar cases are recorded by Sauvages, and also by Fred. Hoffmann, in his Dissertation, *De Siderum in corpora humana influxu medico*. Bruce, in his Travels, states that lunar influence is manifest in the epileptics, who are very numerous in Sennaar.

† Baglivi. Op. Cit. p. 230.

‡ Moseley, Op. Cit. p. 641.

firmed by FLOYER and BENNET. It is more especially in the last quarter or after full moon, says Bennet, that the paroxysms of asthma come on.

As regards the menstrual secretion's dependance on lunar influence, we have a right to be sceptical, though we must not forget the avowed fact of its being more abundant, and even occasionally coming on twice a month in countries nearest the equator, where it is admitted that the moon's action is most uniform.

Fluor Albus lasting for years, has often regular returns at every new moon, the discharge constantly lasting some days.\*

Nephritic paroxysms frequently observe lunar periods; and many cases are on record of fits of the gravel and suppression of urine every full moon. Consistently with my wish to give the present essay a practical bearing, I shall here introduce a case recorded by Meade which fell under his own notice. "A young lady, fourteen years of age, of a good complexion, was from her infancy afflicted with this odd disorder. A day or two before the full moon she waxed pale, weak, dejected, and melancholy; and then unwittingly discharged a large quantity of urine in her sleep. And this discharge continued five or six nights together: after which it ceased, and then her colour and chearful temper returned. The best strengthening medicines were of no avail, until proper evacuations were thought of and made two or three days before the return of the paroxysm, which prevented the lunar influence."

Tumours are often found to vary in their size, according to the lunar periods. A young girl, says MAURICE HOFFMANN, born of an epileptic mother, felt during the time of the moon's increase a singular swelling of the abdomen, which, together with the pains it excited, diminished with the decrease of the moon. Moseley relates the case of a friend of his, an eminent surgeon at Kingston, Jamaica, "who had for many years a monthly enlargement of the liver, which in-

\* Pitcairne, quoted by Meade.

creased with the moon, and after her full, regularly subsided in a bilious diarrhœa.”\* He died the day after the full moon on the 23d of May, 1777.

In no form of disease has lunar influence been more observed and acknowledged than in the hæmorrhagic, whether the discharges arise from a primary irritation of the organ, or are vicarious. “Doctor Pitcairne’s own case is remarkable, both in regard to the disease and its concomitant circumstances. In the year 1687, being at a country seat near Edinburgh, in February, on a fairer day than usual at that season, and the sun looking reddish, he was seized at nine in the morning, the very hour of the new moon, with a sudden bleeding at the nose, after an uncommon faintness. And the next day, on his return to town he found that the barometer was lower at that very hour, than either he, or his friend Dr. Gregory, who kept the journal of the weather, had ever observed it: and that another friend of his, Mr. Cockburn, professor of philosophy, had died suddenly at the same hour by an eruption of blood from the lungs; and also five or six others of his patients were seized with hæmorrhages.”†

Moseley, among many instances which fell under his observation, speaks of two, as more peculiarly worthy of being recorded. A man in Burleigh street, in the Strand, (London,) had a cough for some time, which brought on an hæmoptysis. This continued for six weeks, and then degenerated into a regular monthly eruption of blood from the lungs. He disgorged about eight ounces of blood every full moon. He was always low-spirited and weak, as he expressed it, on the day preceding the hæmorrhage.

The case of Captain Richard Boyle, of the 3d regiment of English guards, affords, to use the language of our author, “one of the most decisive examples of lunar influence in medicinal history.” He was attacked first on the 20th January, 1785, in London, with pulmonary hæmorrhage, and on the following year he was advised to go to the south of France. The journal of his disease for this year exhibits regular at-

\* Op. Cit.

† Meade.

tacks of hæmoptysis, on the very day, or within a day of the new and full moon, from February to August, in the different parts of France and England where he was at these respective epochs. The last three hæmorrhages came on at the instant the moon appeared above the horizon. Even when the hæmorrhage was stopped, the expectoration was as constantly streaked with blood as the moon made her revolutions.

As to relapses, or returns of hæmorrhages from the lungs, the same respectable writer avers, that he never saw one at any other period; unless where too much exercise, intemperance, or accident had occasioned it.

The instances of hæmorrhoidal discharges at lunar epochs are so numerous, and the fact has been so often admitted by both patients and physicians, that we need not at this time cite authorities in its support.

That *fevers* are materially modified in intensity and duration by lunar influence, would seem to be proved by innumerable observations from the time of Galen to the present day. The scepticism so fashionable during the last century, has retarded our information on this head by leading physicians to deny the moon's power over the animal œconomy, and neglect the only means of settling the question, viz. by sedulously watching the course of diseases and their periods of exacerbation and remission, and the corresponding planetary changes and atmospherical constitutions. We ought not in our enlightened and liberal profession to act the part of mere soldiers, by exclusively trying to multiply our means of attacking disease, to the entire overlooking of our more philosophical and statesmanlike duties in warding off and preventing the recurrence of the dreaded evil. We would be very slow in awarding our approbation to the surgeon whose days were occupied in inventing and improving instruments to the neglect of pathological anatomy, and the successive changes of an injured part, which to be seen would be for him the signal for its extirpation, heedless of consequences, and only attentive to multiply his operations: and yet we are too prone to imitate this contemptible conduct, in

hunting after new medicines, and persuading ourselves with having found specifics, the use of which, however short they may come of our first hopes, need not be reprobated had they the merit of those of olden time, in being harmless, or to use the more cant language of medical gossips and posset-making housewives, "of doing no harm if they do no good."

By the rules of philosophy, which permit the positive evidence to prevail over the negative, we are at present in a measure compelled to admit lunar influence in fevers, since hundreds assert positively their having seen it distinctly and unequivocally marked at certain epochs, while, very few, if any, similarly circumstanced, have arrived at a different result, from observations made with a view to this particular point.

Hippocrates, in advising his son *THESSALUS* to study numbers and geometry, says that the knowledge of astronomy is useful to a physician, as the rising and setting of the stars affect diseases. Passing over the opinion of Aristotle on the moon's influence, as too general for our present purpose, we come next to Galen, who, after remarking, that as the sun describes a complete year, so the moon describes a month, by four quarterly changes of seven days each, goes on to say, "that the air which surrounds us undergoes mutations at the time of the new moon, no mortal can be ignorant; and more especially those among whom these things are considered; such as husbandmen and mariners, who calculate on the state of the weather, according to the wind that blows when the moon first appears after her conjunction." The presage of the termination of fevers, and the event of crises from the quarters or potential periods of the moon, every seven days, was the doctrine of Galen, founded on the belief just enounced.

The cause of crises, says *AVICENNA*, is, that the moon varies the operations of nature in diseases.

The remarks of *PAREY* and *QUERCETANUS* on the effect of the moon in plague, have a more specific bearing, when they assure us, that this disease was most liable to attack at the time of the full moon, and that it was most fatal when it came on in the wane.

LORD BACON, a believer in lunar influence, always fell into a syncope or swoon, whenever the moon was eclipsed, though he did not so much as think or know of the eclipse. BALLONIUS relates similar effects on people in health, during the eclipse of the moon in December, 1573: and that there was incredible changes and violent perturbations in the sick; and many were suddenly seized with convulsions and delirium, on the same night.

DIEMERBROEK tells us, that in the plague at Nimeguen, in 1636, "two or three days before and after the new and full moon, the disease was more violent; more persons were seized at those times than at others; and those who were then seized almost all died in a very short time. Many patients who appeared before to be slightly affected, *nescio qua viri-um labefactione oppressi*," says the author, "by an unaccountable decline of strength, sunk and died in a few hours."

Similar effects are recorded by Ramazzini, in his treatise on the epidemic constitution of the years 1692, 1693, and 1694, in the city and neighbourhood of Modena. During these years a pestilential purple fever prevailed, "and it was worthy of observation," says he, "that the disease raged more violently after the full moon, and especially in the dark quarter, and abated upon the appearance of the new moon; as not only I, but other physicians here, constantly observed; and this observation was of great service both in the prognostic and cure." The same author, in another place, has these words. "What happened January 21, 1693, was very surprising. For the moon having been eclipsed that night, the greatest part of the sick died about the very hour of the eclipse, and some were even struck with sudden death."

Moseley, after having given the experience of these and many more authors, furnishes some striking examples of the greater violence and mortality of disease at lunar epochs. In 1780, all the soldiers in the military hospital in Jamaica, under his care in dysentery and intermittents, almost constantly relapsed at the lunar syzygies, and at *Up-Park*, in November, several people died suddenly in convulsions, that were seized at the new and full moon, with the pestilential fever,

then making dreadful havoc in the camp. Most of the men attacked at these periods, who escaped death, lost their memory for a considerable time.

In the same year, in the month of April, eighty-seven men out of two hundred, on board of the Hinchinbrooke sloop of war, at *San Juan* river, were attacked by a malignant fever within the space of a few hours in the night, at the full moon, and nearly all of them died. Many of the survivors on this expedition, among the troops, became insane after their fevers.

The most zealous advocate for sol-lunar influence in fevers, and the one who has adduced the greatest number of evidences in support of it, is Dr. BALFOUR.\* In his work on this subject he gives in reply to his circular, the experience of seventy persons, most of them physicians and surgeons, in proof of the power of the sun and moon in conjunction, in bringing on attacks of intermittent and remittent fevers, or in producing relapses. Among other testimonies are those of Drs. BLANE, HUNTER, JACKSON, and JAMES LIND. The latter, indeed, made a subsequent recantation of his faith in lunar influence, in favour of "the increased effluvia disengaged from the shores and neighbouring marshes at each retiring spring tide." Unfortunately for Dr. Lind, the substituted creed is much more fallible than the original one. Diseases, and more especially remittent fevers, do not arise from effluvia disengaged from the shores or banks of rivers periodically covered and exposed, but from marshes or plains covered by *inundations* and then left to dry; consequently from places not influenced by each retiring tide. It has been found, moreover, that the occurrence or return of fevers at particular epochs in the interior of a country, remote from tide water, however correspondent with the tides, cannot depend on them as a cause, but rather on some primary agent, alike operative on both them and fevers. Many hundred competent witnesses bear testimony in favour of the moon, or the moon and sun in conjunction, being this primary agent.

\* Treatise on Sol-Lunar Influence in Fevers. Sec. Edit. London, 1795.

Is it a sufficient refutation of this belief to deny it, because it seems theoretical, and we have not ourselves seen it—that is to say, because in fever we have not attempted the only true method of testing its truth or fallacy, by cautious observations made with this specific view.

Dr. Balfour accounts, by the separate and combined action of these two planets, for the exacerbations, anticipations and postponements of periodical fevers; and goes far to explain the greater violence and danger of many diseases, particularly fevers, at the periods of the equinoxes, when the sun and moon then exert their greatest power over our planet. We give the author's "inference" from his own and his correspondents' experience.

"Between things that are not connected as cause and effect, a constant and regular coincidence in time and proportion, in so many different points, and in so many different examples, for months and years together, is a circumstance which does not occur in the history of nature; and, therefore agreeable to reason, is a coincidence that can never be expected to happen.

"Upon these principles every constant and regular coincidence in nature, necessarily implies the existence of a proportionate corresponding cause. We are therefore led naturally and irresistibly to refer those that we have discovered and described to sol-lunar influence, which, from its action on the other parts of nature, appears to be perfectly adequate to this effect, and at the same time exhibits *intentions*, *remissions*, and *postponings* in the exertion of its power, *coincident in time*, and *correspondent in degree*, with those which take place in the paroxysms of fevers.\*

"Upon these premises we establish a proof of the operation of sol-lunar influence in fevers, which cannot be rejected without violating the principles and rules from which we infer the existence of a cause in every other question and transaction in life."

\* "Causas rerum naturalium non plures admitti debere quam quæ et veræ sint, et earum phænomenis explicandis sufficient. — Newton Regul. Philosoph. I."

We are assured by the same author "that there is to be observed by any person who will take that trouble, in the appearance and consistence of the urine in fevers, a constant and regular fluctuation; that is to say, regular diurnal and septenary change in its character, coincident and correspondent with the periodical intentions and remissions of sol-lunar influence."

Among the more recent supporters of sol-lunar or luni-solar influence in fevers, we find Dr. JAMES JOHNSON, who cannot be accused of exhibiting any great share of credulity in his various writings. In speaking of the causes of fevers he holds this language. "The last predisposing cause which I shall mention is the influence of the sun and moon. However sceptical professional men in Europe may be, in regard to planetary influence in fevers, &c. it is too plainly perceptible between the tropics to admit of a doubt. I have not only observed it in others, but felt it in my own person in India, when labouring under the effects of an obstructed liver."\* In the same volume, p. 124-5, he quotes the observation of Captain WILLIAMSON, author of "*East India Vade Mecum*," who says, "It is by no means uncommon to see persons, especially Europeans, who have to appearance been cured of jungle or hill fevers, as they are called, and which correspond exactly with our marsh fever, laid up either at the full or change of the moon, or possibly at both, for years after."

Precise and positive as are the evidences adduced in the present essay, they may still be opposed or explained away by reasonings very plausible, and counter-evidence seemingly accurate. Dr. HORSLEY in a very ingenious paper in the *Philosophical Transactions of London*, vol. lxxv. part ii. endeavoured many years ago to refute the commonly received opinions respecting the influence of the moon on the *weather*: and more recently OLBERS, in a paper in the *Annales de Chimie de Physique*, for I think 1822, goes still farther, and denies that the moon exerts any decided effects on the *weather*, on vegetation, or the health of man. We could have

\* On Tropical Climates, Vol. I. p. 112. Phil. 1824.

wished for a little more argument, and a less expeditious manner of dismissing the alleged facts and observations of the advocates of luni-solar influence, than is exhibited by this last mentioned writer, who ought to be very sure of the soundness of his position before he roundly asserts,—“It is here as in many other cases of reverie—we only see it when we *believe* it.” Were we inclined to put entire confidence in OLBERS’ observations, made as he tells us with this express view, and to attribute his results to the northern climate which he inhabits, and in which there is so much to modify the moon’s power, we are still arrested by the observations of MUSCHENBROEK, who says that the weather, in the winter time in Holland, changes and varies almost regularly, and after an almost uniform manner, according to the phases of the moon.

The French physicians generally are sceptical as to the moon’s agency in diseases, whether in fevers or in mental alienation. Moseley anticipating the opinions and language of phrenology, tells us, “By the disagreement and want of connexion we often see among the mental faculties, it is evident they do not dwell in the *same house*. I have seen the moon make a great commotion among the offspring of the *anterior part* of the brain, which could not have happened with the intervention of the other powers”—whereas ESQUIROL, the successor of PINEL at the Salpêtrière or great hospital for the insane at Paris, used to tell us in his lectures that the disturbance produced by the full moon among maniacs was owing to her light, and that if this was carefully excluded, there was no exacerbative movement more observable than at other epochs. VIREY\* holds on the subject of the moon’s influence a cautionary language, which I with pleasure introduce on this occasion. “As to the influences attributed to this satellite on our humours and diseases, we think that a distinction ought to be made, which has not been hitherto done with sufficient care, between the influences of the night itself, of the horizontal position, of incubation and

\* Page 621. Op. Cit. Dict. des Sciences. Med. Art. Lune.

sleep, and the effects which can only be attributed to the moon alone.

“In the first place, the horizontal position invites a greater congestion of blood in the brain; it is manifest then, that apoplexies and even epileptic paroxysms, oppressions of the head, and deliriums, will be more frequent when persons are in bed than during the day time, without the moon having any participation in such effects; moreover, the absence of light during the night, the relaxation of the external organs by sleep, increase oppression among the hydroemics, as hydrothorax, humoral asthma and anasarca, as we see in the sick on the approach of night. The moon does not then seem to exert a direct influence in these circumstances. It is the same with the greater number of typhoid fevers, and those of a bad character, which have their paroxysms during the night, owing to the absence of stimuli, such as light. Hence it is that people could believe in the moon’s governing these maladies as well as the night.”

This is a very plausible explanation, but we are to remember that the causes here given recur at every diurnal cycle, while the increase of the maladies, or the coming on of the paroxysms at the lunar epochs, remains still unaccounted for, if we deny the moon’s influence.

In reference to the greater number of deaths at the moon’s changes, Dr. Moseley says, “Almost all people in extreme age are attacked with their death illness, or die at the new, or at the full moon. In consumptions and chronical diseases it is commonly the same.

“Of the young and middle aged, and those who die of acute diseases, or the result of accident, no estimation can be made correctly—death being often retarded, or accelerated, by interruption or medicine. But even here, if they do not die at the syzygies, they generally die at the quadratures.”

In our investigation of this subject, we must admit the tendencies of numerous causes counteracting lunar influence, in the same way as similar causes, such as inherent strength of constitution, or a highly artificial manner of life, or the taking powerful medicines counteract the effects of atmospherical

changes, winds, humidity, electricity, &c. The man in robust health may condemn lunar influence and laugh at the invalid's dread of an easterly wind; but we know the effects of the latter are not the less certainly injurious, as we have also a foundation for believing that the former is decidedly operative.

In conclusion, I would solicit the members of the profession, in the various parts of the United States, to keep a register of the sick under their care, the duration of the disease, the times of well marked critical perturbations, and of the termination in death. By this means, in a few years such a mass of testimony might be accumulated, as to settle forever the question of sol-lunar influence, and the extent of the periodicity of diseases in general.

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ART. VI. *An Account of some Eudiometers of an Improved Construction.* By ROBERT HARE, M. D. Professor of Chemistry in the University of Pennsylvania.

IN the second volume of the American Journal of Science, I published an account of some Eudiometers, operating by a mechanism which, previously, had not been employed in Eudiometry. A graduated rod, sliding into a tube through a collar of leathers, soaked in lard, and compressed by a screw so as to be perfectly air tight, was employed to vary the capacity of the tube, and at the same time to be a measure of the quantity of air, or of any other gas, consequently drawn in or expelled. About one-third of the tube was occupied by the sliding rod. The remainder, being recurved, and converging to a perforated apex, was of a form convenient for withdrawing measured portions of gas from vessels inverted over water, or mercury.

There were two forms of the sliding rod Eudiometer, one designed to be used with nitric oxide, or with liquids absorbing oxygen; the other, with explosive mixtures. The

latter differed from the Eudiometers for explosive mixtures, previously invented, in the contrivance for exploding the gases, as well as in the mode of measuring them; a wire, ignited by galvanism, being substituted for the electric spark, as the means of inflammation.

I shall proceed to describe several Eudiometers, operating upon the principle of those above alluded to, with some modifications suggested by experience. Fig. 1. represents a hydro-oxygen Eudiometer, in which the measurements are made by a sliding rod, and the explosions are effected by the galvanic ignition of a platina wire, as in an instrument formerly described, excepting, that the method then employed of cementing the platina wire, in holes made through the glass, having proved insecure, a new and unobjectionable method has been adopted.

Fig. 1.

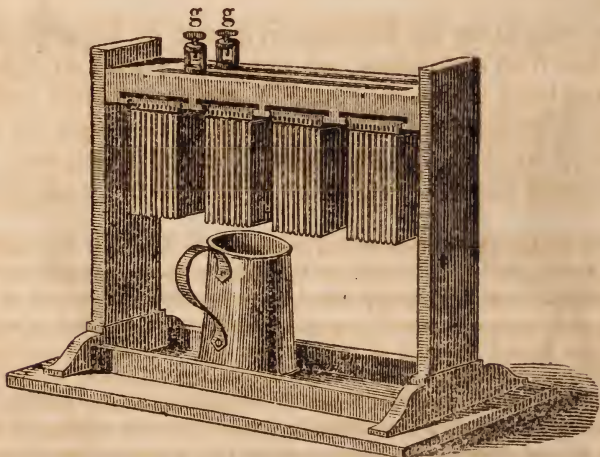


In the instrument represented by the preceding cut, the igniting wire is soldered into the summits of the two brass wires, (WW,) which pass through the bottom of the socket, (S,) parallel to the axis of the glass recipient, (G,) within which they are seen. One of the wires is soldered to the socket, the other is fastened by means of a collar of leathers,

packed by a screw, so that it has no metallic communication with the other wire, unless through the filament of platina, by which they are visibly connected above, and which I have already called the igniting wire. The glass has a capillary orifice at the apex, (A,) which by means of a lever and spring, (apparent in the drawing,) is closed, unless when the pressure of the spring is counteracted by one of the fingers of the operator. The sliding rod, (seen at R,) is accurately graduated to about 320 degrees.

So easy is it to manipulate with this instrument, that any number of experiments may be performed in as many minutes. The ignition of the platina wire is caused by either of four calorimotors, each consisting of four plates of zinc, and five of copper. They are all suspended to one beam as may be seen in fig. 2. following.

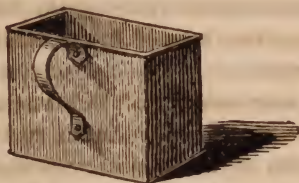
Fig. 2.



Two furrows are made in the wood of the beam, one on each side. These are filled by pouring into them melted solder, after having caused a metallic communication between one furrow and all the copper surfaces of all the four calorimotors; also between all their zinc surfaces and the other fur-

row. The acid, for exciting the plates, is contained in the jug below, which may be so uplifted as to surround with acid either of the calorimotors. Hence while one is in operation, the others are by repose, recovering their igniting power.

Fig. 3.



Or by using a vessel, (Fig. 3.) large enough to receive, and containing acid enough to excite, two of the calorimotors at once, the igniting power may be doubled. The vessels for holding the acid are made of copper, covered with a cement of rosin, rendered tough by an adequate admixture of mutton suet.

In order to use the Eudiometer, it must be full of water, and free from air bubbles, and previously proved air tight,\* the rod being introduced to its hilt, and the capillary orifice open, in consequence of the pressure of the finger on the lever by which it is usually closed. Being thus prepared, let us suppose that it were desirable to analyze the atmosphere.

\* To prepare the instrument and prove it to be in order, depress the glass receiver below the surface of the water in the pneumatic cistern, the capillary orifice being uppermost, and open; draw the rod out of its tube, and return it alternately, so that at each stroke, a portion of water may pass in, and a portion of air may pass out. During this operation, the instrument should be occasionally held in such a posture, as, that all the air may rise into the glass recipient, without which its expulsion, by the action of the rod, is impracticable. Now close the orifice, (at the apex A,) and draw out a few inches of the rod, in order to see whether any air can enter at the junctures, or pass between the collar of leathers and the sliding rod. If the instrument be quite air tight, the bubbles extricated in consequence of the vacuum produced by withdrawing the rod will disappear, when it is restored to its place. This degree of tightness is easily sustained in a well-made instrument.

Draw out the rod 200 measures ; a bulk of air, equivalent to the portion of the rod thus withdrawn, will of course enter at the capillary opening ; after which the lever must be allowed to close it. Introduce the recipient into a bell glass of hydrogen, and opening the orifice, draw out the rod about 100 degrees ; close the orifice, and withdraw the instrument from the water. Apply the projecting wires, (WW,) severally to the solder, (in the two furrows in the beam, fig. 2. communicating with the poles of the four calorimotors, then raise the jug so as that it may receive one of them, and subject it to the acid. By the consequent ignition of the wire, the gas will explode. The instrument being plunged again into the water of the pneumatic cistern, so that the capillary orifice, duly opened, may be just below the surface ; the water will enter and fill up the vacuity caused by the condensation of the gases. The residual air being excluded by the rod, the deficit will be equivalent in bulk, to the portion of the rod remaining without ; and its ratio to the air subjected to analysis, may be known by inspecting the graduation.

In the case of the gaseous mixtures above described, the deficit has, in my experiments, been 126 measures. Whereas, according to the theory of volumes, it ought to be only 120. But I have not as yet operated with hydrogen, purer than it may be obtained from the zinc of commerce ; and some allowance must be made for the carbonic acid of the air, which may be condensed with the aqueous vapour produced by the oxygen and hydrogen.

In the invaluable work on the Principles of Chemistry, lately published by Dr. Thomson, it is suggested, that in order to obtain correct results in analyzing the air with the hydro-oxygen Eudiometer, more than 42 per cent. of hydrogen should not enter into the mixture. I am not as well satisfied of the correctness of this impression, as I am generally with the results of the wonderful industry and ingenuity displayed in the work above-mentioned.

If oxygen is to be examined by hydrogen, or hydrogen by oxygen, we must of course have a portion of each in vessels

over the pneumatic cistern, and successively take the requisite portions of them, and proceed as in the case of atmospheric air.

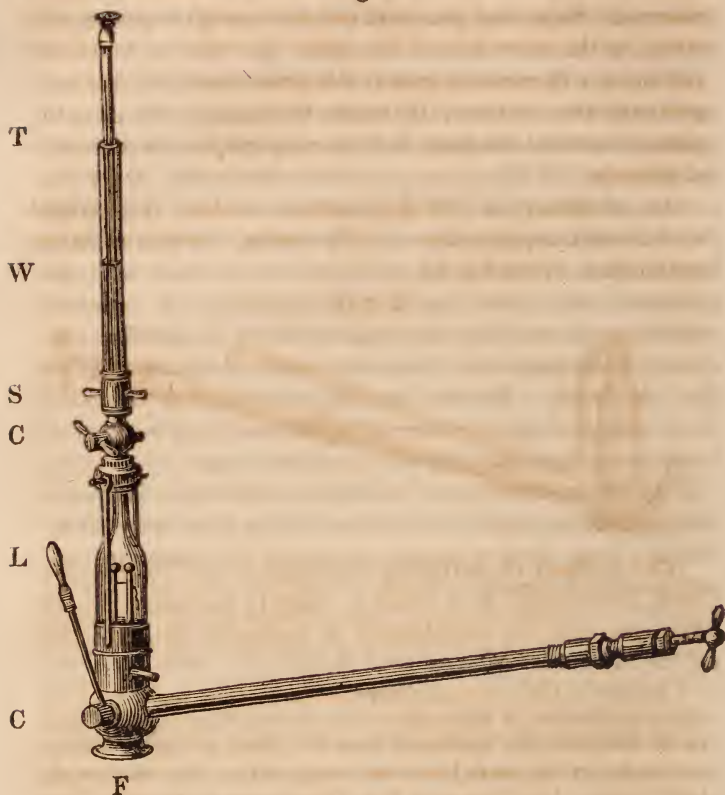
B, fig. 1. represents a glass with wires inserted through small tubulures, in the usual mode for passing the electric spark; should this method of producing ignition be deemed desirable for the sake of varying the experiments, or for the purpose of illustration. This glass screws on to the socket (S,) the other being removed. The wires, (WW,) remain, but should be of such a height as not to interfere with the passage of the electric spark. The instrument is operated with as usual, excepting the employment of an electrical machine, or electrophorus, to ignite the gaseous mixture in lieu of a calorimotor. For the travelling chemist, the last mentioned mode of ignition may be preferable, because an electrophorus is more portable than a galvanic apparatus.

In damp weather, or in a laboratory where there is a pneumatic cistern, or amid the moisture arising from the respiration of a large class, it is often impossible to accomplish explosions by electricity.

#### *Of the Mercurial Sliding Rod Eudiometer with a Water Gage.*

The Eudiometer which I have described, though satisfactory in its results, and in its conveniency, when used with water, has not been found so when used over mercury. The great weight of this fluid caused the indications to vary in consequence of variations of position, during manipulation, too slight to be avoided. The instrument represented in the following cut, (fig. 4.) is furnished with a water gage, which being appealed to, enables us to render the density of the gases within, in equilibrio with the air without. Hence we can effect their measurement with great accuracy.

Fig. 4.

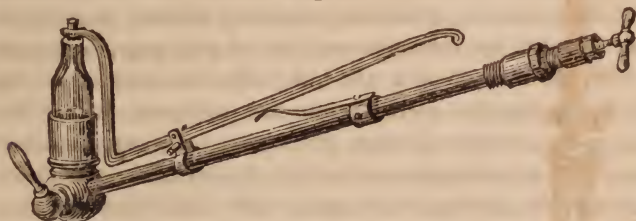


Let us suppose that this Eudiometer has been thoroughly filled with mercury, the sliding rod being drawn out to its greatest extent; and that it is firmly fixed over a mercurial cistern in the position in which it is represented in the drawing, the little funnel-shaped part at the bottom, descending into the fluid to the depth of half an inch. Above this part is seen a cock, (C,) the key of which, in addition to the perforation usual in cocks, has another, at right angles to, and terminating in, the ordinary perforation. When the lever, (L,) attached to the key of this cock, is situated as it is seen in the drawing, the tube containing the sliding rod commu-

nicates with the recipient, but not with the mercury of the reservoir. Supposing the lever moved through a quarter of a circle, to the other side of the glass, the tube in which the rod slides will communicate at the same time with the recipient and the reservoir. By means of the gage-cock, (C,) the passage between the gage and the recipient is opened or shut at pleasure.

As subsidiary to this Eudiometer, another is provided with a rod and graduation exactly similar,\* excepting its being shorter. (See fig. 5.)

Fig. 5.



The method of analyzing atmospheric air by means of these instruments, is as follows. Supply the subsidiary Eudiometer with its complement of hydrogen gas, by intro-

\* In order to ensure accuracy in the measures of gas, made by the subsidiary Eudiometer, it is necessary to attend to the following precautions. In the first place, the instrument must be proved air tight, and free of air bubbles, by the means prescribed already in the case of the Eudiometer for water. (See note, page 89.) The presence of air bubbles is always indicated by the extent of the vacuity which appears, when the glass recipient is held uppermost, and which disappears when it is held lowermost, the weight of the mercury acting upon the elasticity of the tubes, always causes a minute change; but by the smallest bubble of air, the effect is very much augmented. The Eudiometer should be introduced into the vessel whence the gas is to be taken, and about ten per cent. more than is necessary drawn in by opening the orifice and duly drawing out the rod. The Eudiometer being lifted from the mercury, with as little change of position as possible, the rod may be adjusted accurately to the point desired. A momentary opening of the orifice causes the excess to escape. The gas thus measured and included, is then easily transferred to the principal Eudiometer, by introducing the apex of the subsidiary instrument under the funnel, (see F, fig. 4.) opening the orifice, and forcing the sliding-rod home.

ducing the apex, of the glass recipient, into a bell glass containing, over mercury, the gas in question, and drawing out the sliding rod, the orifice being kept open only while above the surface of the mercury, and inside of the bell.

The gage-cock, (C, fig. 4.) of the principal Eudiometer being closed, and that which opens a communication between the recipient and the funnel F open, and the instrument having been previously thoroughly filled with mercury, and placed over the mercurial cistern, as already mentioned, introduce into it, through the funnel, the gas which had been included in the subsidiary instrument, (fig. 5.) Next shut off the communication with the mercurial cistern, re-establish those between the recipient and the rod and gage, and push the rod into its tube up to the hilt. The re-entrance of the rod, by raising the mercury into the recipient, forces the hydrogen in bubbles through the water of the gage, and displaces all the atmospheric air which it previously contained. Now shut the passage to the gage, open that which communicates through the funnel with the mercurial cistern, and draw out the rod to its utmost extent. Into the Eudiometer thus situated and prepared, introduce successively 100 measures of hydrogen and 200 measures of atmospheric air, by means of the subsidiary Eudiometer: then closing the passage to the mercurial cistern, and opening the passage to the gage, push in the rod, until the water in the gage indicates that the pressure on the gages included is equivalent to that of the external air. The gage-cock being closed, the gases are ready to be exploded. The explosion is produced by galvanic ignition, as in the case of the Eudiometer for water, (fig. 1.) excepting that instead of carrying the Eudiometer to the calorimotor, the circuit is established by lead rods severally attached to the galvanic poles, by galleys and screws, (see gg, fig. 2.) One of the lead rods terminates in a piece of iron, immersed in the mercury, the other is fastened to the insulated wire of the Eudiometer. Under these circumstances, one of the calorimotors is surrounded with the acid contained in the jug, and an explosion almost invariably succeeds. Before effecting the explosion, the number of the

degrees of the sliding rod, which are out of its tube, should be noted; and it must afterwards be forced into the tube, in order to compensate the consequent condensation of the gases, as nearly as it can be anticipated. A communication with the gage must then be opened gradually. If the water is disturbed from its level, the equilibrium must be restored by duly moving the rod. Then deducting the degrees of the sliding-rod, remaining out of the tube, from those which it indicated before the explosion, the remainder is the deficit caused by it; one-third of which is the quantity of oxygen gas in the included air. Or, the residual air being expelled by the rod, and the quantity thus ascertained deducted from the amount included before the explosion, the difference will be the quantity condensed.

It may be proper to mention, that as other metals are almost universally acted upon by mercury, the cocks, sockets, screws, and sliding-rods of the mercurial Eudiometers, are made of cast steel. The tubes containing the rods, are of iron.

Since the drawings, (figs. 1. and 4.) were made, verniers have been attached to the screws, through which the sliding-rods pass; so that the measurements are made to one-tenth of a degree.

I have alluded to the water gage without explaining its construction. It consists of three tubes. A small tube of varnished copper, (which is fastened into the only perforation which communicates with the cock, and of course with the glass recipient,) passes up in the axis of a glass tube, (T, fig. 4.) open at top, cemented into a socket, (S, fig. 4.) which screws on to the cock. A smaller glass tube is placed in the interstice between the external glass tube and the copper tube in its axis. This intermediate glass tube is open at its lower termination, but at the upper one is closed or opened at pleasure by a screw. The interstices between the three tubes are partially supplied with water, as represented in the drawing, (W, fig. 4.) When the passage between the gage and the recipient is open, if the pressure on the included air be more or less than that of the atmosphere, the water will rise in one

of the gage tubes, and sink in the other. Other liquids may be substituted for water, in the gage, when desirable.

In addition to the principal collar of leathers, and screws for rendering that collar compact, there is in the mercurial Eudiometers, a small hollow cylinder, (a piece of a gun-barrel,) with an additional collar of cork for confining oil about the rod, where it enters the collar of leathers; otherwise in operating with mercury, the leathers soon become so dry as to permit air or mercury to pass by the rod.

It may be proper to point out, that in operations with the hydro-oxygen Eudiometer, accurate measurement is necessary, only, with respect to one of the gases. In analyzing an inflammable gas by oxygen gas, or oxygen by hydrogen gas, it is only necessary that the quantity of the gas which is to be analyzed, and the deficit caused by the explosion, should be ascertained with accuracy. The other gas, which must be used in excess, sometimes greater, sometimes less, must, in using the Mercurial Eudiometer, be made to occupy the gage. In analyzing the air, or any mixture containing oxygen, the gage is filled with hydrogen gas, as already stated; but, in examining inflammable gas, the atmospheric air may be left in the gage, as its only active qualities are those of oxygen gas.

Figs. 6. and 7. represent those forms of the sliding-rod Eudiometer, which I have found most serviceable for experiments with nitric oxide gas; with the solutions of sulphurets; or those of sulphate, or muriate of iron, saturated with nitric oxide.

Fig. 6.

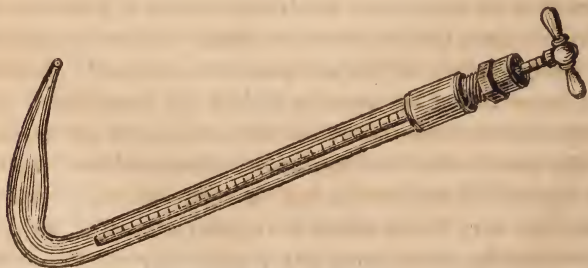
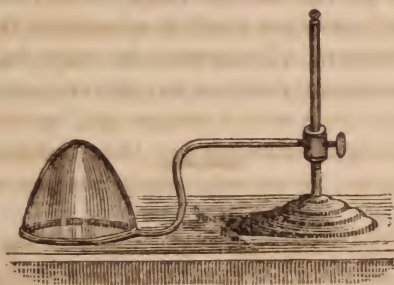


Fig. 7.



The receiver, (fig. 8.) shaped like the small end of an egg, is employed in these experiments, being mounted so as to slide up and down upon a wire.

Fig. 8.



This vessel being filled with water, and immersed in the pneumatic cistern, the apex being just even with the surface of the water, one hundred measures of atmospheric air, and a like quantity of nitric oxide, are to be successively introduced. The residual air may then be drawn into the Eudiometer, and ejected again into the receiver through the water, to promote the absorption of the nitrous acid produced. Lastly, it may be measured by drawing it into the instrument, and ejecting it into the egg-shaped receiver, (fig. 8.) or into the air, when the quantity of it will appear, from the number of degrees which the sliding-rod enters during the ejection. That in this way gas may be measured with great accuracy, may be demonstrated by transferring any number of measures, taken

separately, into the semi-oval receiver, and subsequently re-measuring them.

The Eudiometers, (figs. 6 and 7.) with the accompanying semi-oval glass vessel, (fig. 8.) may be employed with the dissolved sulphurets, or with solutions of iron, impregnated with nitric oxide in the following way. Let a small phial, with a mouth large enough freely to admit the point of the Eudiometer, be filled with the solution to be used. Introduce into the bottle, over the pneumatic cistern, 300 measures of the air or gas to be examined. Transfer the bottle, still inverted, to a small vessel containing water, or a quantity of the absorbing fluid used in the bottle, adequate to cover the mouth of the phial, and compensate the absorption. When there has been time enough for the absorption to be completed, transfer the residuum to the receiver, (fig. 8.) and measure as in the case of nitric oxide.

As soon as I can make a sufficient number of satisfactory observations with the various Eudiometers, of which I have now given an account, I will send them to you for publication.



ART. VII. *Observations on the Effects of Drinking Cold Water.*  
By WILLIAM CARLL BREWSTER, M. D.

AS on this subject so little is written, and the junior part of the medical community are nearly without chart or compass, depending almost entirely upon verbal instruction, or the suggestions of their own minds to guide their practice; in the hope of contributing to a better knowledge of this frequently fatal accident, I venture to lay before the reader the results of my experience.

Placed as I have been in the extreme part of the city, my practice necessarily confined for the most part to the labouring classes of society, among whom its occurrence is most frequent, I have had an excellent opportunity of making extensive observations and inquiries. With the experience of

two summers, in the last of which I met with nearly thirty cases, and devoted considerable time and attention to the subject, I feel some degree of confidence in the correctness of my views.

The prevalence and mortality of the disease under consideration, is always in proportion to the heat of the weather. In the last summer, the warm season commenced unusually early. June was very oppressive, and July was almost insufferable. The latter was the most fatal month in the whole summer, as will be seen by the following statement:—In the week ending July 16th, the deaths in Philadelphia were 129—in New York, 197: in the week ending July 23d, the deaths in Philadelphia were 180—in New York, 184: in the week ending July 30th, the deaths in Philadelphia were 163—in New York, 207. A just estimate of the influence of the temperature upon disease can be formed by comparing the mortality of a warm with that of a cool season. In the present summer there were 180 deaths in the week ending July 30, whereas in the corresponding week of the former year, there were, I believe, but 66 deaths, an astonishing increase of almost three-fold. So much for the general mortality.

The whole number of deaths from cold water during the last summer in this city, was 22. In New York there were 69 sudden deaths reported in nine days, commencing July 16th, most of which were supposed to be occasioned by cold water. In Boston, we are told, that about 54 cases of the same complaint occurred to one physician in a week. When we recollect that many of the deaths reported “sudden,” “unknown,” “apoplexy,” “coup de soleil,” &c. might have been owing to cold water, we may fairly infer that the mortality was much greater than is stated. In order to show the close connection existing between the heat of the weather and this disease, I will give a table of the state of the thermometer at the Health Office for one week, commencing July 17th, 89°—18th, 89°—19th, 91°—20th, 91°—21st, 93°—22d, 92 $\frac{1}{2}$ °—23d, 94°. This was by far the hottest week we had, and in it, as we should be naturally led to suppose, fourteen of the twenty-two cases of death from drinking cold wa-

ter occurred. The obvious deduction, therefore, as before stated, from these premises is, that the prevalence of the disease is exactly in proportion to the degree of heat.

In my experience but two varieties of this affection have occurred, and these, I believe, to be the regular forms in which it presents itself. The first ensues where a large quantity of *very* cold water has been swallowed, while the body is "preternaturally heated." In this case an immediate shock is communicated to the nervous system which it is unable to withstand. The patient is perfectly paralyzed. The respiration is laborious, the eyes are sunken, the blood forsakes the surface, and engorges the heart and larger viscera, the skin is pale, the lips livid, and the patient dies in two or three minutes. The effect is similar to that produced by lightning: and it is somewhere stated, that as in that case, putrefaction commences very shortly. This is no doubt the reason why the relatives of the deceased wish the body interred almost immediately; and this is the cause, which to my utter regret, has rendered abortive my most anxious and untiring solicitations to examine the bodies of such patients after death. These cases of very sudden death are comparatively rare. I have, however, seen a person drop dead under a pump after drinking cold water.

But the following is by far the most common form in which we meet with this disease. Soon after the patient has swallowed the cold water, he feels an uneasy sensation about the scrobiculus cordis. This sensation gradually increasing, is soon converted into violent lancinating pains, which extend all through the abdomen and thorax. Breathing produces as much distress as in pleurisy, and the patient describes his pain as if a cord were bound tightly around his stomach, or sharp-pointed instruments were penetrating the parts affected. As the case advances, the pains become more intense, and assume a regular spasmodic form, with intervals of ease, between them. In short, at this stage the case very much resembles labour in the stated contractions which it causes. Being suffered to advance, all the symptoms are aggravated. The throes are now excruciating; the patient bends himself

almost double in order to obtain relief; or rolls and tosses about from one side to the other, writhing under the most painful contortions, and in the most indescribable agonies. If no assistance is afforded, the patient next becomes phrenitic. The pulse, though in some instances thready, is very hard when examined closely; the whole surface is hot; the face is excessively red; the eye-balls glare; there is a wild look and vociferous delirium, and, in short, all the marks of determination to the brain. These symptoms gradually subside, and the patient falls into a lethargic coma and lies perfectly easy. The jaws are now fixed; deglutition is interrupted; respiration is slow and stertorous; the pulse is gone; there is a sluggish circulation and cold sweats; pallid surface, and singultus generally closes the scene.

But if by the timely interposition of appropriate remedies, the malady becomes arrested, violent reaction commonly ensues, producing a febrile condition of the whole system, and topical congestions, particularly of the brain.

There are many causes which may predispose to an attack of the disease. Exposure to the direct rays of the sun, by prostrating the whole system, but especially the stomach, will enable the cold water to act with more certainty. The effect produced on the stomach by heat, we see abundantly confirmed by the loss of appetite and disagreeable taste in the mouth during warm weather. But there is another manner in which exposure to the sun acts—by concentrating a portion of the circulating fluids upon the brain, the full engorgement of which is always the secondary effect of cold water. It is my firm belief, that the cold water affection and genuine ictus solis, do frequently exist in one and the same person, the incipient state of the latter always favouring the former complaint. I am led to this conclusion, by recollecting that a great proportion of the cold water cases of the last summer, happened among the labourers on the Vine street sewer, who were exposed to the exhausting rays of a meridian sun, while at the depth of eight or ten feet below the surface of the earth, and who, in a state of debility and diaphoresis, would

come up to the pumps and drink off one or two pints of very cold water.

Intemperance is another predisponent cause of this disease: and this is the reason, as we shall hereafter explain, that it is so prevalent among habitual drunkards. I know very well, however, that this fact is denied by some, who suppose they have sufficient protection in brandy from the danger of drinking cold water: because that article constitutes an important ingredient in the treatment of the complaint. But such should recollect that it is not the nature, but the temperature of the liquid taken, that produces the evil; I state it as a fact, that cold brandy and water, cold beer, or any other cold article, will produce the same effects under similar circumstances, as pure water itself.

What is the nature of this singular affection? Various speculations have been hazarded on this subject; some of them too on very groundless premises. But there is one fact which cannot have escaped the observation of those who have bestowed much attention upon such cases. Upon placing the hand over the abdomen of a patient suffering from drinking cold water, the stomach will be felt like a ball, quite hard, and in fact very much resembling the contracted uterus, after its contents have been expelled. The nature of the complaint, therefore, is to me rendered evident.

In cases where a large quantity of cold water has been swallowed, producing immediate death, I am inclined to attribute the effect to the overpowering shock the nervous system receives, similar to an electric shock. In the ordinary forms, however, the cold water, by virtue of its sedative powers, exerts a paralyzing influence over the stomach; this nature opposes; and in the conflict to overcome it, a spasm in the muscular fibres of the stomach is excited. If this effort succeeds, there is convulsion of the whole system, and the blood is propelled with force upon the brain, causing the congestion in that organ which is always observed in these cases, and which it is the business of the physician to modify by a timely interference. To elucidate these views I will subjoin some facts.

That cold is a sedative, and as such produces debility, is undeniable; debility being thus caused, spasm next ensues. Now by what process this is brought about, it is not easy to say. But there is not an axiom in medicine better established, than that spasm is the sequel of debility. Every one that has had much medical experience, must have seen tetanus produced by the debilitating operation of cold. In syncope, where the circulation is almost suspended, spasmodic twitchings are generally observable; and lastly, in the lowest forms of typhus gravior, the patients often die in convulsions.

The fact, therefore, of the agency of debility, in producing spasm is unquestionable; and with regard to the disease under consideration, any thing that tends to cause debility, increases the susceptibility of the stomach to affection from cold water. This is the reason why a state of habitual inebriation predisposes to an attack. Liquor taken in small quantities, is undoubtedly a tonic and stimulant; but if indulged in to excess, it weakens the powers and impairs the functions of the stomach. This theory, therefore, accounts very satisfactorily for the known fact, of the prevalence of the disease from drinking cold water among the intemperate.

Of little service, however, would speculations be to mankind, did not the legitimate deductions therefrom lead to some important results.

In the first form to which I have alluded, there is seldom any time for medical interposition; or if the physician does arrive before the vital spark has fled, it is but to see the inefficacy of human means.

In general, however, the patient suffers some time, before he is aware of the nature of his complaint, constantly flattering himself with the hope that it is a colic affection, which will subside spontaneously. It is three or four hours therefore before the physician is sent for; and when he comes in, he finds his patient racked with excruciating pains, between which there are distinct intervals of ease. He is perfectly sensible, and can generally tell the probable cause of his malady. The pulse is sometimes full, strong, and frequent; but mostly it is small, corded, and depressed, and in some in-

stances almost imperceptible. Whatever be the state of the pulse, however, at this stage of the disease, there can be little doubt as to the proper course to be pursued. My practice is, whenever I am called in at this time, always to draw off a large quantity of blood; and when permitted to detract a sufficiency, have never failed in curing the complaint. In general twenty or thirty ounces will be required at the commencement, and the operation repeated if there is no abatement of symptoms.

While bleeding is going on, as the patient can always swallow, something should be administered by the mouth. "I know of but one certain remedy," says Dr. Rush, "and that is liquid laudanum." Though this is generally *certain*, yet it is not the *only* remedy. Its exhibition, however, is at all times indispensable. Large quantities are to be given at stated intervals. I commonly combine the laudanum with some of the stimuli, as ether, spirit of camphor, spirit of ammonia, &c. This latter article may be very advantageously given with the laudanum in doses of a teaspoonful each, every ten minutes. This is apparently a very large dose: but the disease it must be remembered is itself extremely violent.

The patient is generally thirsty, and calls loudly for drink. This therefore affords an opportunity to increase the effect of the remedies by adding some stimulant, and brandy is the proper article. It may be taken raw or diluted in almost any quantity; in one instance I saw a quart given in about an hour with evident advantage.

While we are thus pouring our remedies into the system, we are not to neglect external applications. Early in the disease we should prepare a mustard plaster, and as soon as the bleeding is finished, we should apply it over the epigastrium. It ought to remain on from thirty to fifty minutes, or even longer, if it appears dilatory in producing its effects.

By the judicious use of the means mentioned, all the symptoms gradually subside, and the patient becomes for a time free from pain. Generally, however, febrile symptoms supervene, with determination of blood to the head, flushed face, blood-shot eyes, delirium, &c. This state requires a repetition of the operation of venesection, the application of

cups and cold water to the forehead and temples, and sinapisms to the soles of the feet. Some smart purgative then commonly completes the cure.

By a paper published in Boston, emetics are recommended in the treatment of this complaint. This practice is contrary to all my ideas of the disease; and if it could operate beneficially at all, it must be in the commencement, and then only with a view of evacuating the offending matter.

In order to produce a more perfect and expeditious vesication, the application of nitric acid to the region of the stomach has been proposed. In one instance I have known boiling water to be poured over the epigastrium with alleged benefit. To confine this to the part a common muffin ring may be held on the surface, and the boiling water poured into it.

In the commencement of these remarks it was observed, that the disease from drinking cold water appeared to be much more common in New York and Boston than in this city. This may at first sight appear strange, as its prevalence is universally dependent on the heat of the weather. But it must be recollected that in this city we have access at all times to river water, which may be drunk in any quantity, under almost all circumstances, without producing evil consequences. But in the northern cities they do not enjoy this advantage, and the labourers are obliged, in order to allay their thirst, to drink cold pump water, which in a heated state of the system is almost certain to generate the disease in question.

The following are the directions given by the Humane Society of this city, to prevent the occurrence of this dangerous accident :—

1st. Grasp the vessel out of which you are about to drink for a minute or longer, with both your hands.

2d. If you are not furnished with a cup, and are obliged to drink by bringing your mouth in contact with the stream that issues from a pump or a spring, always wash your face and hands previously to your drinking with a little of the cold water.

[The estimable author of the ensuing essay, transmitted it to us for publication, not long before his death. As we had recently given to our readers an article on the same subject, we thought it best to postpone it for a season. It is now inserted, as well from its intrinsic merits, as forming a record of the talents of our deceased friend, whose life too early for the interests of medical science was cut off.]—*Editors*.

ART. VIII. *On the Nature and Medical Properties of Ergot.*  
By JOB G. OSLERE, M. D.

THIS substance, which has not been long introduced into the *Materia Medica* in England, is called “horned or spurred rye.” Dr. CULLEN, in his *Materia Medica*, has applied to it the name of “*Secale Cornutum*;” while by the French it is called “*Ergot*,” by which name it is now generally known throughout the world. The name adopted by Cullen is generally employed as the technical designation of this substance.

Although the use of this substance, as a specific for the production of uterine action, had been long traditionally known among the German inhabitants of Pennsylvania, and possibly of other states, the medical profession are indebted to Dr. STEARNS, of Saratoga Co. New York, for the first correct account of its medicinal properties. He first dispersed the clouds of fear and prejudice, which for ages had enshrouded its name, and gave to the world, from his own experience, one of the most valuable remedies we possess.

The production of this substance, constitutes a very curious and interesting subject of inquiry; and the naturalist might profitably investigate its origin; but wishing as briefly as possible to state the results of my experience, I shall give the observations I have made, without advancing any mere speculations.

Ergot is the result of a peculiar disease in rye, which is one of the four diseases of plants, enumerated by LINNÆUS, and by him denominated *Clavus*. This disease very often attacks rye in France, and in some of the provinces it is espe-

cially predominant: in wet seasons it is occasionally seen in Great Britain and other parts of Europe. In this country the rye is also liable to the same disease; in new settlements, there is always, I believe, more or less of it to be found among the grain; but it is less commonly met with in fields that have been constantly cultivated for many years.

It has long since been ascertained that many gramineous plants, and some of the grasses, are liable to this disease; but rye appears to be most susceptible to its influence. Wheat is affected in a similar manner, but the ergot, or substance produced is not so large, and according to my experience, is not so active. For medicinal purposes, that from rye answers best, as it is larger, and produced in greater abundance.

Nothing satisfactory has been assigned to explain the causes of this disease. I am, however, disposed to believe, that it depends upon a specific contagion, which is generated in a light rich soil, in rainy weather, and that its sphere of action is limited to a very short distance, probably not exceeding two or three feet; and that it does not affect the grain at any other time than when it is in bloom. I have been led to this conclusion, from having observed, that if a grain of ergot be placed on a head of rye when it is putting forth its blossom, the husk on which it is placed, as well as several others, will be filled with this substance. I have tried several experiments of this nature, and uniformly obtained the same results.

In my first experiment with this article, I took several spurs of ergot, and placed them in different parts of a small lot of rye, at the time it was putting forth its blossom, fixing one spur of ergot in a husk, in different situations; and wherever the ergot was placed in due time, it was followed by a crop of that substance, as well from several neighbouring husks, as the one to which the spur had been attached: but the glume which contained the spur produced the greatest quantity of ergot; and the number in each husk gradually diminished, as their distance from the central spur increased. In every situation where I had placed a spur, I observed the same thing to take place.

Being desirous to ascertain whether any effect would be produced on the grain, by placing a spur of ergot, at the root of the stalk, I placed several grains of this substance at the roots of several stalks of rye, at the time when they were in full bloom, but no effect whatever was observed on them, as in due time all were filled with sound rye.

In another experiment, I selected several stalks of rye, which were growing promiscuously around a stack of straw, and some distance apart. In one of these I placed a spur of ergot, and then pulled up several stalks that were very near to it; I left one stalk on the north side of the one in which I had placed the ergot, distant three feet; and also one on the south side, distant two feet six inches. From the head in which I had placed the spur, in due time I obtained twenty-two grains of ergot, and from the head on the south side I also obtained five grains; but the stalk on the north side was not in the least affected, which, I think, clearly proves that the contagious nature of this disease of the grain, is limited to a very short distance. Similar experiments, tried by my friends in the country, invariably produced the same results.

From the preceding experiments, I have drawn the following conclusion, relative to the production of this substance. The ergot is the result of a peculiar disease in rye, which disease is generated in a rich light soil, during wet weather, and can only act upon the grain when in bloom; that at such time it extends itself by contagion to several of the adjoining stalks, going from one to another, and might, if the grain continued in bloom for a sufficient length of time, pervade the whole field.

That it acts upon the rye only when in bloom, I am convinced by experiment. If a spur of ergot be placed in a husk of rye when the blossom is decaying, it will have little or no influence upon it; in some rare cases I have found the grains of rye a little shrivelled; but if, on the contrary, it be placed there when the blossom is just putting forth, it will completely pervade the husk to which it is attached, as well as several adjoining ones, all of which will be found filled with that substance. From experiments repeatedly tried, it does

not appear to affect the grain at any other time than when in bloom.

Ergot, the result of this vitiated state of the rye, is an elongated excrescence, which occupies the place of the seed in the husk, and is found projecting from among the spiculæ, or ears. It is of an irregular, curved form, having three lines, or grooves, running longitudinally on its surface, and when broken, presenting somewhat of a triangular appearance. It is of a dark brown colour externally, and a dull white within; it has an unpleasant taste, is of a brittle texture, and very much resembles in shape the spur of a cock.

If the ergot be large, generally there are but few on an ear, and the grains of rye in the same ear, are not quite so large and full as those of another; the stalk is strong and vigorous, but when the grains of ergot are small, there are many on the ear, and the stalk is less strong and thrifty. Four or five of these grains are commonly found upon an ear, frequently ten or twelve, and sometimes even twenty. If exposed to the air, ergot dries readily, becomes less in size, and very light; a measure that holds fourteen pounds of rye, will hold but nine pounds of ergot.

Although its medicinal properties were not made public until a very late period, yet the ergot was known as far back as the year 1596, and assigned as the cause of several bad forms of disease, which, at that time, prevailed to a great extent among the poorer classes of society, in various parts of England and France. "Some of these put on the symptoms of epilepsy, and shortly destroyed the subjects of their attack; others were characterized by very desperate and extensive gangrene of the limbs, preceded by great vertigo and symptoms of drunkenness."

By far the most distressing of these complaints was that of dry gangrene, which has appeared in England at various periods subsequent to the year 1596, and in a great majority of cases proved fatal; for as yet no means are known by which we can arrest its progress. During the late war between the United States and Great Britain, our troops, while stationed on the Canadian frontier, were much affected with dysentery

and diarrhœa, the cause of which was ascertained to be the daily use of bread made of flour containing this substance. That these affections were ascribed to their true cause appears probable, from the fact, that when a supply of flour arrived from Ohio, they entirely disappeared, and did not return, so long as the troops were supplied with good flour, but when their stock was exhausted, they again had recourse to the flour of the country, the dysentery again made its appearance, and prevailed to an alarming extent, continuing its ravages, as long as bread made of the country flour was used.

Never having experimented with this article on the healthy system, I have little to say of its effects in this condition; but from the experiments of Dr. ERSKINE and others, it appears slightly to increase the fullness and frequency of the pulse; produces a glow over the surface; excites nausea, pain and giddiness in the head, and sometimes vomiting. Its effects, when given in small doses, appears to be that of a stimulus; but when administered in large doses, its ultimate operation is to reduce arterial action, by exciting nausea.

From the effects of this article on the healthy system, we should not be induced to estimate its powers highly; but in this, as in many other cases, we cannot judge of the effects of a medicine in disease, from its action on the healthy system. When given to the male sex it is characterized by no peculiar action, but when administered to the pregnant female, it displays the most singular properties. Upon the uterus it expends its whole force, exciting in that organ the most violent pain and contractions; hence its use to the judicious practitioner. When the uterus, languid and debilitated by long-continued and ineffectual efforts to expel its contents, ceases to perform its ordinary functions, leaving the tender female in a situation the most wretched and deplorable of which we can conceive, it is then that the practitioner feels the importance of this invaluable adjuvant, as it is to such cases our remedy seems peculiarly adapted, and displays its most valuable properties. In the hands of a judicious practitioner, it will often supersede the necessity of using instru-

ments, which are ever alarming to the mind of a parturient female. I have no doubt, that by a judicious administration of this medicine the lives of many children may be saved, who otherwise must necessarily fall a sacrifice to the imperfections of our art.

The ergot has long been known to the empiric midwives, and used by them for the purpose of expediting lingering labours, but we have no account of its having been used in regular practice, previous to its introduction by STEARNS, (to whom the profession are deeply indebted,) who, after a fair and impartial trial of the article, in many cases, recommends it to the world in a tone of great confidence. In his letter to Dr. AKERLY on this subject, he speaks of its virtues in the following terms:

“The prejudices which you observe exist in Europe against the use of ergot, as being actively injurious or contemptible for its inactive qualities, I know to be totally unfounded. I have administered it to more than one hundred parturient patients, and I have never given it, except in cases that threatened a difficult or lingering labour. I do not recollect a single instance in which it did not ultimately succeed; and I have generally been able to predict from the commencement of its operation, with tolerable accuracy, the period of delivery. This satisfactorily proves to me its active qualities. That it is not injurious you will have some reason to believe, when I assure you, that I never lost a patient that I attended during her parturition, neither immediately, nor in consequence of sickness thereby induced; and that I have never had any case where the disorder could be traced to this source. I find it is more active when prepared by decoction, than in powder. I therefore always prefer the former. It is much to be regretted, that scientific physicians have generally held in contempt every medicine that quacks have been in the habit of using. When we reflect that accident has given origin to the use of our most active medicines, and that we are indebted to empiricism for their most useful qualities, we certainly should neglect no opportunity of deriving aid to science from this source.” “I believe that the ergot, if properly used, will produce abortion in any stage of pregnancy. I

have not, however, sufficient experience to be positive on this subject."

That the ergot is capable of producing abortion in animals at any period of utero-gestation, my own experience as well as that of others, has fully persuaded me. Dr. ERSKINE has very accurately detailed the result of several experiments which he tried on cats, at various periods of pregnancy, and, in every instance, he succeeded in producing abortion. Being myself desirous of ascertaining whether this substance was really possessed of the properties ascribed to it, I tried, during my residence in the country, in the summer of 1818, some experiments on animals for this purpose. The first was performed on a sow, she was supposed to be in her seventh week of pregnancy, and in every respect appeared to be doing well. I ordered her to be shut up in a close pen in the morning, without being fed, and at 10 o'clock, A. M. I gave her one drachm of ergot in powder, mixed with a little sour milk. In about three hours afterwards, the animal appeared to be in great pain, as was evinced by her squealing and endeavouring to get out of the pen; these pains continued violent until about 5 o'clock, P. M. when they appeared to abate in violence, and the animal became more easy. I then repeated the same dose in decoction, mixed with some bran, which she devoured with great eagerness; and in the course of one hour afterwards, she was again seized with pains, which continued lasting and violent until ten o'clock, when she was again examined, and left for the night. Upon visiting her the next morning, I was much surprised to find that she had aborted during the night, nine small pigs, which were about the size of common mice. The sow had by this time completely recovered from the effects of the medicine, and continued to thrive until she was killed for pork. In the course of a few days after this experiment, I had an opportunity of performing one of a similar nature on a cow, that had been turned out to fatten, and was supposed to be with calf. From the success of the medicine in the former case, I resolved to try its effects in the present instance, although not certain of her being pregnant. I accordingly had her put

into a stable in the morning, and kept there without food until ten o'clock, when two ounces of ergot were given in powder, and she was left: as I had occasion to go from home, I did not see her again till 4 o'clock, P. M. when I found her in the greatest agony, which she evinced by bellowing, alternately laying down and getting up, and twisting her body in every possible direction. I again left her, and did not see her till 6 o'clock, when I found her in the very act of aborting, which was completed in a few minutes after I arrived at the stable. The abortion was about the size of a common *full-grown rat*, but very imperfectly formed. The pains continued for about three hours after the abortion, when the animal became more easy, and ate of some hay that was presented to her. The next morning she appeared debilitated from the exertions of the preceding evening, and in a great measure to have lost her appetite, which, however, she completely regained in the course of three or four days, and continued to thrive as well if not better than before the operation.

My next experiment was performed on a cat, that appeared to be near her time of delivery. I gave her sixteen grains of ergot in powder, mixed with butter, at eight o'clock in the evening, and confined her in a room: on visiting her about eight o'clock the next morning, I found her delivered of four kittens, which were still living. The cat appeared to have suffered but little from the operation of the medicine, and was very attentive to her kittens, all of which however died during the day.

From the result of the preceding experiments, I have not the least hesitation in believing, that the ergot is capable of producing abortion at any period of utero-gestation. It appears to have a specific determination to the gravid uterus, and although of incalculable value to the judicious practitioner, it would nevertheless prove, in unskilful or designing hands, the most dangerous substance with which we are acquainted; and ought, therefore, on all occasions, to be moderately and carefully used.

In those cases, where from an original smallness of the

pelvis, or other circumstances, a difficult labour is anticipated, we may resort to the ergot with the utmost advantage, for the purpose of bringing on premature labour, by which means we will often be relieved from performing the horrid operation of embryulcia, which, though of but rare occurrence, is nevertheless too often performed. I have no doubt that the lives of many children have been sacrificed, that in the hands of a judicious and skilful practitioner might have been saved. For my own part, in all cases where I suspected danger to the mother or child, by suffering her to go to the full period of utero-gestation, I would prefer bringing on premature labour with the ergot, by which means the mother would be relieved of much unnecessary suffering, and the child have a greater chance for its life.

This plan has been carried into actual practice by several persons of the highest respectability. Professor JAMES, in his lectures on this subject, relates the case of a woman, who had a pelvis naturally so small, that it was impossible for a full grown foetus to pass through it; she had been several times pregnant, and in every case the operation of embryulcia had been resorted to for her delivery. In a subsequent pregnancy, Dr. J. in consultation, suggested the propriety of bringing on premature labour; this was agreed to, and performed with success, both to the mother and child: the woman has since been pregnant several times, and in every instance the same plan was pursued.

In difficult and lingering labours, where the energy of the uterus has been expended by long-continued and ineffectual efforts to expel its contents, leaving that organ weak and debilitated, the parts being dilated, and the child remaining high up in the pelvis, we may resort with the greatest advantage to the use of ergot. A dose of fifteen or twenty grains of this substance, given in powder or decoction, under such circumstances, will in general completely restore the lost action of the uterus, and enable it to expel its contents. The powder is decidedly preferable in these cases, as the effects of the decoction are in general too sudden, violent, and sooner expended; when given in powder, the pains come on more gra-

dually, continue longer, and are in general sufficiently energetic for the purpose of delivery.

I am acquainted with several respectable practitioners, who are in the habit of using it, in all cases of difficult labour, where there is a want of action in the uterus, and who speak in the highest terms of its efficacy. I have been informed by my friend and preceptor, Dr. HUMPHREY, of Delaware County, that he has used it in more than a hundred cases, with the greatest possible benefit, and bad consequences have never resulted from its use, neither has he ever lost a single patient in parturition.

Dr. AKERLY, in his letter to Dr. DEWEES, recorded in the twelfth volume of the New York Medical Repository, speaks in the highest terms of the ergot, in cases of lingering labour.

In cases of retained placenta, where the energy of the uterus has been expended by previous exertions, the remedy will prove equally beneficial: a few grains of ergot, administered under such circumstances, will excite an expulsive action in the uterus, sufficient for the purpose of expelling the placenta, without occasioning any very violent after pains.

The reports of practitioners, on the virtues of the ergot, in cases of retained placenta, are exceedingly numerous, and well calculated to inspire us with confidence in the use of the remedy.

In those cases of uterine hemorrhage, that depend upon a relaxed and debilitated state of the uterine fibre, much benefit may be expected from its employment. It will excite a contractile effort in that viscus, which, in general, will put a stop to the flow of blood. I once saw a case which had resisted all the remedies that are generally resorted to in these cases. Notwithstanding the application of astringent injections, and the internal use of large doses of *saccharum saturni*, the blood continued to flow in large quantities: as a last resort, five grains of ergot were given in decoction, and in less than thirty minutes, the hemorrhage ceased, and the patient recovered, though she was much debilitated from the loss of blood she had sustained.

ART. IX. *A short account of Dr. ROUX's "Memoire sur la Staphyloraphie ou Suture du Voile du Palais."* By JOSEPH G. NANCREDE, M. D.

PRACTITIONERS have for a long time been aware, that there exists occasionally an imperfection in the operations of nature, which by leaving an opening in the velum palati, impedes, more especially in infants, the process of nutrition, and in adults sometimes totally precludes pronunciation. The inconveniences arising from this division of the palate are numerous, and of considerable importance; at the very dawn of life it threatens it with extinction by the obstacles it opposes to the healthful operations of lactation, and as the individual who is thus unfortunately situated, advances in life, the infirmity is gradually developed and severely felt.

Heretofore this malformation was considered beyond the resources of surgery, for no one it appears had ever entertained the hope of remedying the afflicting deformity. In fact, the disease itself, considered as one of very rare occurrence, attracted but slightly the attention of practitioners of surgery. Chance, to which we owe so many discoveries, introduced to Dr. Roux, a distinguished practitioner and professor of pathology in the Faculty of Medicine of Paris, a young graduate of that school who laboured under the effects of the deformity. An examination of the parts immediately suggested the mode of remedying the malformation, which the ingenious professor has since very happily put in practice, and with the most gratifying success.

Nearly six years have elapsed since the author first conceived and performed this simple, but new operation. With a degree of prudence highly creditable to his modesty as well as to his judgment, he has delayed publishing his new method, until further trials placed beyond the reach of doubt the practicability of an operation, which is now guaranteed by several successful experiments. It is therefore clearly established, that by means of a methodical operation, similar

in its effects to that performed for the cure of the hare lip, the velum palati may recover its natural form, as well as the power of exercising the functions for which it is designed by nature, as completely as if it had never been imperfect. The extent of the deformity and other circumstances, may, in some instances, prevent a complete success, but in others an entire removal of the disease may be accomplished.

The importance of the new operation will depend in a great measure upon the number of unfortunate cases, requiring its performance, which are to be found in the community at large. Considered in this light, it assumes a rank among the most useful operations of the surgical art, for its value becomes daily more appreciated, as many cases are brought to light by the very knowledge of the possibility of a cure, which otherwise would have remained in total obscurity.

Independent of the particular malformation we allude to, other deviations from the usual shape of the palate, involving the solid texture of the roof of the mouth occasionally occur. In cases where the two bones were separated the suture of the soft parts has been accomplished, with evident benefit, though the success in these cases could not be as complete as in those, whose soft parts alone are the seat of the disease.

The case which is first reported by Dr. Roux, and the one which inspired him with the idea of the operation, is that of a young physician, aged five and twenty years, who was born with a complete division of the velum palati. His pronunciation was so extremely defective, that he could not converse in an audible manner, without the greatest difficulty. Upon a careful examination of the fauces, the velum palati was found completely divided in its whole length, and the two distinct halves of this organ constantly separated, forming a triangular space, corresponding to the isthmus of the throat.

The object which the operator proposed to attain, was to bring together the two edges of the division, then to excite inflammation, by the process used in the operation for the

hare lip, and to keep the parts in close contact for a space of time sufficient to secure adhesion.

He commenced the operation by carrying three ligatures through each division of the soft palate, by means of a crooked needle; with the assistance of these ligatures, the parts were secured and drawn downwards, so as to allow the operator to cut off the edges, as is usual in the hare lip operation. Having accomplished this, nothing further was required, but to unite the parts, by drawing the ligatures as tight as was advisable. The whole process, which was necessarily more tedious than difficult, lasted fifty minutes. At the expiration of twelve days, the patient was enabled to read in an audible voice, before the Academy of Sciences, a short account of the operation he had undergone, and the next day he left Paris, on a visit to England. Six months after the operation had been performed, he revisited Paris, and his voice and pronunciation had then experienced so much evident improvement, that it presented but a very slight difference from what it is in men, who are gifted with the best organs of speech.

The operation has since been performed twelve times by the same surgeon. Seven cases presented simple divisions of the soft palate. Five of this number have completely, or perhaps it will be more strictly correct, to say almost entirely recovered the natural tone of voice: the remaining two cases have not derived the same benefit from the operation, in consequence of their own imprudence, in making at too early a period, too much use of the parts. In both these cases, the operation performed again, under more favourable circumstances, may secure all the advantages derived in the first five cases. The remaining six patients were unfavourable cases for the success of this operation, all of them presenting in addition to the division of the soft palate, more or less separation between the bones composing the roof of the mouth. One, however, of these cases, has reaped some benefit from the operation, but all the others remain perfectly as they were before its performance.

The great difficulty encountered in these cases, appears to have been to prevent a disunion of the parts, after adhesion had apparently been formed. In several, after days had elapsed with every prospect of union, the parts gradually returned to their former state. It is therefore to cases where the soft parts alone are the seats of this malformation, that Dr. Roux's new operation is adapted, and in such cases its success appears now established beyond doubt.

## CASES.

ART. X. *Case of Extra Uterine Pregnancy.* By WILLIAM  
JOHNSON, M. D. of New Jersey.

MRS. ELIZABETH COVERT, aged forty-four, of small stature, and delicate constitution, has borne three children, the youngest of whom is now twenty years of age. She menstruated regularly until the beginning of December last, when she again became pregnant. [She has never miscarried.] Suppression of the catamenia, morning sickness, and the usual symptoms of pregnancy occurred.

Called upon to visit her in the middle of the month of January, I found her labouring under much pain in the hypogastrium, great tenderness to the touch in the region of the womb, considerable degree of prolapsus uteri, and bowels somewhat constipated. Her urine was evacuated without difficulty; her pulse but little excited. The remedies prescribed by me afforded a temporary relief; but she continued to have frequent and severe attacks of pain in the lower part of the abdomen. These attacks were generally brought on by some considerable bodily exertion.

I paid her a few visits in the months of January and February. In May I again called upon her, and was informed by her that she had "felt life" some time, and that the motions of the child were altogether in the right side. A considerable degree of obscurity had in my opinion rested upon this case from its commencement, and feeling very much embarrassed in accounting for the peculiar symptoms manifested, and wishing to satisfy myself more fully, I requested another examination. This examination tended in no degree to remove the obscurity of the case; for in applying my hand to the abdomen I found the uterus scarcely developed enough to contain a placenta alone: the finger introduced per vaginam discovered the os uteri rather more anterior, and lower down

than natural. I stated plainly to my patient that I had my doubts about her being pregnant, but apprehended the uterus from its considerable enlargement contained *something*—what? I could not determine, and wished her to take other professional advice. My request was not complied with.

About the middle of July I met my patient near her own house, and she expressed much uneasiness about her situation, again informed me that all the motions of the child were in the right side, that she suffered very much from pain, particularly from a “lump” in the fore part of her body which incommoded her very much in stooping, and that it was generally sore to the touch. [This “lump” I afterwards found was the enlarged uterus.]

In the afternoon of the 15th of August, I was called upon to attend Mrs. C. who was now supposed to be in labour. Upon my arrival she told me that she had overrun her reckoning, that she had within a few weeks suffered much from pain, but that her pains since morning had been severe, and more like those of labour. These pains commenced in her back, extended across her abdomen, and recurred at intervals. By touching, I now satisfied myself, that my patient's pregnancy was *extra-uterine*. My opinion was founded on the following circumstances. Introducing my finger within the vagina, it came readily in contact with the head, (which presented well, and upon which I could easily trace the sutures and posterior fontanelle,) but instead of the usual thickness of the womb, there seemed a remarkably thin interposing substance; the vagina was so unusually shortened that the finger could readily pass over every part of its surface, still I found great difficulty in discovering the os uteri, but by careful examination it was found *immediately under the symphysis pubis*, and dilated so as to admit with ease two fingers. *The finger could pass into the uterus, which was found expanded and reaching nearly half way to the umbilicus. The hand could grasp it:* it was not contracted. The head in descending had passed between the vagina and rectum, and thus thrown the uterus forward. Instead of the regular tumour of the abdomen occurring in natural pregnancy, there was

here an oblong tumour in the right side, of a very irregular surface. These irregularities I soon found were produced by the child's limbs, *which could be traced with remarkable ease*. The *left side* of the abdomen was tumid, but soft; the foetus was wholly contained in the right side. I could not discover that the uterus or its mouth were the least affected by the pains. The head descended a little by the action of the abdominal muscles during the pains. Milk was secreted in the breasts.

The foetal system having been completely developed, there was now evidently an attempt made by nature for its expulsion. This kind of pain, after a continuance of some hours, ceased, and did not again return.

I stated to the husband and female friends my views of this case, and requested a consultation. Dr. STYKER, a gentleman of the first respectability was sent for, who concurred in the opinion I had given of the case being an extra-uterine pregnancy. The loss of a little blood, anodynes, rest and other remedies to tranquillize the system and avert inflammation were prescribed, and we left our patient for the present. In subsequent consultations with him and other professional gentlemen, we came to the conclusion that an operation for the removal of the child was not expedient. We were influenced in forming this opinion by the general sentiment of practitioners, and the results of cases already published. There is little here to encourage, but very much to deter from such operations. The great difficulty in the way, however, was the management of the placenta. We had reason to infer that it must be of large size, and abundantly supplied with blood, and that its separation would be followed by dangerous hemorrhage. In addition to other and weighty objections, our patient was by no means a promising subject for an operation.

For more than three weeks Mrs. C. remained comfortable, by taking small doses of laudanum occasionally. In fact, she informed me that she felt more comfortable than she had been for months before, and until now neither the state of her pulse or abdomen discovered the presence of inflamma-

tion; but this state of things could not be expected to last long—slow inflammation took place—the *left* side became painful and sore, and the motions of the child produced exquisite distress. On the 9th of September her pains became agonizing. The child appeared to have ruptured its envelope, as it was felt in the *left* side—it was the child's motion that produced such distress. My patient took this day about seven hundred drops of laudanum by the mouth and by injection, (principally by the mouth,) before any thing like relief was obtained. I directed a large quantity of the bruised leaves of the stramonium, to be applied to her right side, immediately over the child. The application was renewed. The motions of the child ceased entirely, and I felt satisfied of its death. She obtained comparative ease. There was still much uneasiness of the abdomen, and it became more tumid. Her mouth soon became aphthous, and her system appeared to be sinking under irritation. I was contented with keeping her easy by small doses of anodynes. Complaining much of the bloating of her bowels, I gave her half an ounce of ol. terebinth. combined with an equal quantity of ol. ricini in two doses. It operated two or three times, and afforded much relief, removing greatly the tenderness of the abdomen, and diminishing the frequency of the pulse and heat of the skin.

17th. Continues to fail—much emaciated—great prostration—aphthæ spreading—appetite gone—but pain of the abdomen less.

19th. Is easier, though she has occasionally severe pains—abdomen more tumid—but not so tender—evidently contains water—fifteen drops of laudanum twice a day keeps her easy.

25th. Abdomen much distended—tumour uniform—cannot now trace the limbs of the child as at first—fluctuation very perceptible—aphthæ, debility, and emaciation increasing—fever has never assumed a regular hectic form—pulse about 90.

29th. Debility increasing—emaciation at its greatest extent—the powers of life rapidly giving way—at night died.

Permission could not be obtained to open the body, al-

though repeatedly urged, until the 4th of October, when, after much solicitation, permission was obtained for its disinterment. The inspection was made by Dr. VREDENBUGH, of Somerville, (who had taken a lively interest in the case, and frequently seen the subject in her last illness,) and myself.

In opening the abdomen, about half a gallon of a dark coloured fluid escaped. The child enveloped in its proper membranes, presented itself to our view, situated in the umbilical and right iliac regions, with its head completely wedged in the pelvis. The membranes were entire, and of the usual thickness, and so transparent that those portions of the child in contact with them, could easily be seen through them. The child lay with its breast nearly inclined to the spine of the mother, with its thighs drawn up upon its abdomen, and the legs flexed on the thighs. The membranes were opened, and half a pint of bloody water discharged. A well formed female child was extracted, weighing five pounds three ounces, and measuring when stretched out, twenty-one and a half inches. It was in good preservation—no important change had taken place upon it. The umbilical cord was two feet long, and of the ordinary thickness. The placenta was attached to the left ovary, rectum, and adjacent parts, and firmly adherent. When removed it weighed one pound and a half—it had undergone no change—the uterus was without a blemish—it measured four inches in width at the fundus, and three inches in thickness—the bladder was sound—the effects of inflammation were most obvious in the left side, in the production of preternatural adhesions. Two very important objects were obtained by this examination; first, a confirmation of our opinion as to the precise nature of the case; and secondly, a demonstration of the absolute inutilty of an operation for the relief of the mother. The placenta could not have been separated without her inevitable destruction.

ART. XI. *Singular Cases of Hydrophthalmia*. By D. THEODORE COXE, M. D.

THE following notice of several unfortunate individuals in the same family, who seem destined to be forever cut off from the enjoyment of the most valuable of our senses, will perhaps prove interesting to the readers of this Journal: under this expectation, the facts therewith connected were some time since collected, and would have been given before, but that it seemed desirable to ascertain the effects of the plan of treatment adopted for their relief.

The family alluded to, consists of seven children, four of whom have always enjoyed excellent eye-sight, and have never experienced the slightest disease in those organs. There is considerable variety in the temperament, colour of the eyes, &c. in all seven; no two, either of the affected or unaffected, being perfectly alike in these respects. The parents too, are healthy and stout, with good eyes.

The eldest of the three affected, is now about thirteen years of age, and immediately after an attack of measles, (which occurred when she was six years old, was very violent, and particularly severe upon the eyes,) the present disease made its appearance. It is a species of dropsical effusion, affecting more particularly the anterior chamber, but extending in a slighter degree over the whole ball. The eyes protrude, are occasionally painful, and vision is confined to merely distinguishing day-light from darkness, and being sensible of a luminous spot when a candle is presented to her. A peculiarity in the lachrymal apparatus of this girl, when an infant, was mentioned to me by the mother, nothing of the kind having been observed in any of the other children; viz. that, though like other children she frequently fretted and cried, *she never shed a tear*. This, however, did not last long, and she soon relieved herself like other children. Her general health seems uniformly to have been good.

The second, (also a girl,) is now about nine years old, and has been blind about three years, the watery effusion having

appeared about the same time, as in her sister, but not immediately following any disease of the system. One of her eyes is much more distended and prominent than the other, and far more than either of her sister's, which are nearly of a size. Her general health is also good.

The third, (a boy,) is five years of age, and was attacked when about six weeks old with an affection of the eyes, differing in some respects from that of his sisters. Although the same kind of effusion exists, it is trifling compared with theirs, and would not be perhaps observed, were the eyes not otherwise affected. At the period above mentioned, his eyes became dull and cloudy, till finally a thick film seemed to be spread over their surface, obscuring vision: with one eye, the blindness seems to be total, and in addition, there is a corneal speck of two or three lines in diameter: with the other, however, he can see more distinctly than his sisters. These children do not follow each other in immediate succession, but are separated by some of the healthy ones.

Wishing to avail myself of the well known judgment and experience of Dr. PHYSICK in diseases of the eye, I sent the patients for him to look at, and shortly afterwards was favoured with his opinion on the subject. No operation was thought by him likely to be of any service, (at least at that time,) and he recommended a course of purging, blistering, and low diet to be faithfully adhered to, as he had occasionally seen such a course do good. One case he particularly alluded to, in which after several months the effusion disappeared and never after returned. The doctor, however, was far from being sanguine as regarded these cases. The plan recommended, however, was adopted, and diligently persevered in for nearly three months, without appearing to make the least impression upon the disease, except that the eldest's pupils, which, when I first saw her, were contracted, had at the end of this period dilated considerably. This dilatation too, renders probable what she thought, that she could see more distinctly. The prominence, however, remained the same in her as in the others.

Being now discouraged, and unwilling to continue this

plan, I did not again see them until my return from the country, whither I had gone for a short time. I found them in statu quo, except that their eyes were more painful and inflamed, partly, perhaps, owing to the change in their regimen, and partly to the increased heat and brilliancy of the sun. With some difficulty I persuaded the eldest girl again to give the old plan a trial, as at her period of life, (approaching puberty,) greater changes happen in the system than at any other time. She takes also a small quantity of mercury daily, in hopes that a gradual and cautious salivation may assist the other remedial measures.

Having now stated every thing material with regard to these cases, that has come within my knowledge, I forbear making any remarks or reflections upon them, leaving every reader to draw his own conclusions.

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ART. XII. *Cases of Prolapsus Uteri*. By HENRY PERRINE, M. D. Communicated in a letter to Dr. DEWEES.

SINCE December last I have had six cases of prolapsus uteri, three of which, if not four, furnish remarkable exemplifications of the correctness of the observations, made in your communication published in a former number of the Journal.

Two occurred in the persons of near relatives to a family in which you practised, during their residence in Philadelphia; and the first previous to my seeing your paper, in which the effects of replacement were as striking as those which have since occurred when they were looked for, and consequently were not so surprising to myself.

The first, a married lady, mother of three children, had been attended during the whole of the summer and autumn, and part of the time by two physicians, for a variety of symptoms in the stomach, bowels, heart, and pains in the *right*

side, and was at one time supposed to be dying. She had leucorrhœa and other signs of prolapsus. She had also dysmenorrhœa, large portions of membrane being detached. She had at irregular intervals intermittent fever.

I was called to her on the 13th of December, her attending physician having been previously discharged; and from professional delicacy, forbore inquiries on any part of the history of her case, not immediately connected with her present symptoms. She said that she had been principally treated for chronic hepatitis, but was convinced that her case had been mistaken; that her present symptoms were always her prominent ones; that she had never undergone a manual examination, but was willing immediately to submit to one. I found the neck of the uterus on the perineum, ready to become visible; leucorrhœa present—frequent strangury—inconvenience in passing fæces—costiveness—palpitation of the heart—great acidity and flatulence of stomach—dysmenorrhœal pains, resembling labour in severity, but *periodical*, commencing every other evening, and lasting five or six hours. A pessary, with astringent injections was used; vol. tinct. of guaiacum, in doses to be governed only by their effect on the bowels, or in other words, to operate as a laxative without any other; and for the stomach, (and consequently heart,) I gave a powder of prussias ferri, myrrh, and columbo. The effects were really astonishing. (I had not then read your paper.) The symptoms all subsided rapidly, mental as well as corporeal. In a few days the menses returned, and with very little membrane or pain. In five or six weeks another natural menstruation and impregnation occurred before the next period; three months have now passed since the pelvis instead of the pessary supports the uterus; and she has uniformly, as well as her friends, declared, that her health has not been so good in several years, indeed never better.

I wish however to recur to one or two circumstances in the progress of the treatment. The *periodical* exacerbation of the uterine pains did not appear to me for a few days as being aggravated by, if not entirely made up of a disguised intermittent; but as soon as the thought occurred, I directed an anti-

icipating opiate an hour before the next period, and more frequently repeated doses of the prussiate of iron in the interval; but as it came on an hour earlier than usual, with cold extremities superadded, it was not given. The pruss. ferri was still recommended for the ensuing interval, and the paroxysm again came an hour earlier, with a little heat succeeding the cold extremities, but was shorter and the pains less severe. The next was still an hour earlier, chilliness more generally diffused, succeeded by diffused heat and sweat, constituting a slight chill and fever, shorter than either, and scarcely any pain. It never returned, having vanished, anticipating as regular intermittents sometimes do.

Another circumstance was the retardation of the second menstrual period, apparently by over doses of the vol. tinct. guaiacum. The end of the month had been looked for with peculiar anxiety, and the doses had been increased to a tablespoonful three times a day; for as two-thirds of that quantity at the previous period had been necessary to produce a laxative state of the bowels, it was inferred that an increase of dose was directed with the further view of accelerating menstruation. A week thus passed before I ascertained that a diarrhœa existed, from eight to ten motions a day, and to this I was directed by the alarm occasioned from the accompanying diminution of urinary secretion. As the circumstance had before existed to an alarming degree to her, indeed scarcely any secreted, with a corresponding state of the bowels, I directed her to diminish the dose of tinct. guaiaci, so as to have but one or two motions daily; the urinary discharge returned, and in three or four days the menses appeared.

The tonic powder to which I referred, always had a *very speedy* effect, as much so as diffusible stimulants in checking the *palpitation* of the heart; and as speedy in relieving *acidity* as the alkalies or alkaline earths; while the *propensity* also became subdued permanently, principally under this powder, but finally under iron filings, taken in laxative doses. When I first saw her she ate, as she said, a handful of chalk a day; since that day not a particle of any alkaline antacid.

The iron filings were given on account of the materials for the comp. tonic powder giving out; and the only measure was their effect as a laxative by supposed combination with the acid. I learned the iron-antacid practice from a medical gentleman, Dr. BOWERS, who had it prescribed for him by Dr. RUSH, in his own case, of which acidity and costiveness were the prominent symptoms, and which had resisted every treatment that himself or friends could suggest. Whether these facts, or any part of the case above, are of importance, you can best determine.

The obligations that I am under from the perusal of your writings, would induce me to contribute any mite that my records may exhibit of their value. The vol. tinct. guaiacum, which has had such contradictory testimony of its efficacy, is in high reputation here among men whose recommendation is worth receiving; and your observations concerning the probable causes of its failure have been read with pleasure; but if I mistake not you have omitted one very striking one, the want of virtue in the guaiacum itself. A great deal of that met with in the western shops differs very much from the description given in books, as well as the different samples which I have procured from each other. If it be wrong to condemn the genuine and good Peruvian bark on account of the failures experienced in the use of the powders sold under its name, it is as wrong to condemn the genuine effective guaiacum on account of its ineffectual namesakes. The vol. tinct. guaiacum is becoming reputable in other diseases besides those enumerated by you.

ART. XIII. *An Account of some singular cases of Gangrene.*

By SAMUEL L. HOWELL, M. D. of Woodbury, N. J.

**DURING** the last winter and spring, at a time when the prevailing type of disease was highly inflammatory, there occurred in this vicinity some cases of an anomalous and extraordinary malady. I say in this vicinity, for as far as my inquiries have extended, I cannot discover that a disease at all similar has been noticed elsewhere. It will be best described by narrating the following cases, which are substantially extracted from notes, carefully recorded after each day's visit, whilst the cases were under treatment.

Mary Jenkins, aged thirty-two years, of a robust, masculine frame, and a constitution of more than ordinary vigour, was taken ill on Saturday evening, 28th May last. She is said to have been seized with a chill, which was succeeded by violent fever, soon after which she complained of distressing pain in the calf of her left leg. On the following day, about 4 o'clock, she was affected with a slight convulsion, which, though it did not continue long, was followed by delirium, under which she still laboured at the time of my first visit, viz. about 6 o'clock on Sunday evening. She was then very restless, and was constantly varying her position in bed. Her mind was greatly excited—she talked incessantly, and although incoherently, yet her expressions and manner were such as indicated great distress.

Her skin was hot, but moist, and her pulse, though active, was without much force or tenseness, but from the vigour of her constitution and fulness of her habit, I did not hesitate to use the lancet. Blood was immediately drawn to the extent of  $\frac{3}{4}$ xvj. which produced a considerable effect on the pulse, and brought out profuse perspiration, but it produced no evident improvement in her other symptoms. Left her the following cathartic—Fol. sennæ  $\frac{3}{4}$ ss. and sulph. magnes.  $\frac{3}{4}$ j. to be taken in divided portions, but to commence immediately. Saw my patient again, two hours afterwards. She had now become partially restored to reason, and was consi-

derably tranquilized. She complained much of the pain in her leg, which I found somewhat swollen, and along the inner belly of the gastrocnemius muscle, I felt a hard tumor which occupied nearly the whole of that portion of the muscle, but lay so deep as not to involve the cellular tissue, much less the skin, which was quite free from any thickening or discolouration; the tumour, however, could readily be grasped in the fingers, and was distinctly moveable, but when forcibly pressed it occasioned a good deal of pain.

The cathartic operated very freely during my second visit, and I anticipated speedy relief for my patient. Before leaving her, I directed a blister to be applied to the back of the neck, should the delirium continue. Decoc. canthar. in sp. terbinth. to be freely rubbed on the limb over the seat of the tumour, and small doses of opium and camphor to be given, and repeated at short intervals, until ease and rest were procured.

On the following morning, I found that a most appalling change had taken place in the state of my patient—her limb to my great astonishment, was gangrenous from the seat of the tumour down nearly to the inner ankle—the whole having become of a very dark purple, and the vesicles, (caused perhaps by the decoc. cantharid.) being filled with bloody serum. The foot was quite cold, and the tumefaction and soreness extended now considerably above the seat of the original tumour, and there were deep or purplish streaks, running in the course of the lymphatic vessels up to the groin. The pulse at the wrist was now become feeble, her countenance had a sunk, distressed appearance, and every symptom threatened the most imminent danger.

I ordered the whole limb enveloped in flannels, wetted with the undiluted decoction of cantharides—the same preparation to be rubbed freely and frequently on the other leg. The body and arms to be bathed with strong tinct. capsici. and wine and bark to be administered both by the mouth and by enema. The diminution of temperature in the gangrenous limb, I endeavoured to obviate, by having heated bricks placed around it.

At 4 o'clock, P. M. the period of my fourth visit, I found

my patient's situation desperate. The purple colour and mottled appearance had extended quite up to the knee, and the bloody vesicles were dispersed over the whole of the inner and posterior portion of the leg. Her extremities were now cold—her breathing laborious—her pulse was feebly trickling at the wrists, and every symptom threatened impending dissolution. She was now entirely sensible, and expressed a great anxiety to recover.

The crude bark having been rejected, I substituted the tincture, of which a dessert-spoonful was given with each portion of wine, as often as her stomach would bear it, and I surrounded her limb just above the knee with a strip of blister plaster, about two inches in width. These remedies, however, were alike unavailing. The poor woman died at one o'clock on the following morning, (Sunday,) after an illness of about fifty-four hours. She had been remarkably healthy through life, and so far as I could gather from herself and her friends, she had sustained no injury, which could give any satisfactory clue, whereby to trace the origin or cause of this most extraordinary affection.

In the same neighbourhood, and at the very time I was attending the unfortunate case just narrated, I was called to witness another, which, in its commencement, bore a strong resemblance to it, and threatened to be quite analogous too in its progress and termination. The patient here was a man aged about forty, of a feeble constitution, who was somewhat emaciated by recent disease, (intermittent fever,) and what was still worse, had been subject from an early age to epilepsy. He was attacked with a severe pain about the middle and on the anterior part of the left leg, which from his description seemed deeply seated in the interosseous space. When I saw him on the day after, I found a hard, flattened tumour, which occupied the anterior face of the limb and involved the external portion of the gastrocnemius muscle. There was some discolouration of the skin, but it was of that dark red or purplish hue, which indicated great debility in the vessels, and torpor of circulation. The pain in the part was deep-seated, and was of a dull obtuse kind, which called

forth a continual moaning. He complained likewise of soreness in the arm, in which I had bled him some days before, with the hope to prevent the expected return of the fits. On examination, I found some redness about the wound, which however had completely united, and there was an unusual degree of hardness immediately beneath the integument, which extended to a radius of about two inches around it. He laboured under great anxiety and distress—some nausea occasionally, and constant sighing. His pulse was completely typhoid, and his tongue was dry and chopped, with a peculiar streak of dusky red extending through its centre. I commenced the treatment in this case with small powders of ipecacuanha and calomel, given at short intervals, until they produced vomiting, which gave great relief. They finally moved the bowels also, which were very torpid. The decoc. canthar. was applied to the tumour on the leg, which gave some relief. The same application was made to the arm, and he was directed to take the infus. cinchon. as his stomach and symptoms required, with small portions of the pulv. Doveri to allay his pain. The local application not appearing sufficiently powerful, blisters were applied on the following day, both to the leg and arm, and in the case of the former with more effect. They relieved the pain, and arrested the evident tendency to gangrene. The appearance of the arm, however, still continued unfavourable. The hardened and thickened state of the integuments continued to extend, and now occupied nearly all the anterior part of the fore-arm. The parts immediately surrounding the wound had so far lost their vitality that they might be touched without the patient being conscious of it. About this time a gangrenous spot appeared on the internal condyle of the humerus, which grew black, and finally sloughed. After a few days the most prominent part of the tumour on the leg lost in a great measure its sensibility, and manifested again an evident disposition to run into gangrene, but this was probably prevented by the tonic and cordial plan of treatment which was adopted, and perseveringly pursued. Conjoined with this, in consequence of the extremely dry and unnatural state of his skin and mouth, I gave small por-

tions of calomel, opium and ipecacuanha, with a view to restore the secretory functions, and occasionally I substituted the blue pill with the same adjunct, and exhibited in the same minute portions. Erysipelatous inflammation now seized the diseased arm, producing great pain, and in the course of two days it progressed upwards nearly as far as the shoulder, but this was happily checked by encircling the arm with a strip of blister plaster, and by the free use of the decoc. canthar. Though the pain was allayed, and the progress of the disease arrested, the tumefaction about the elbow continued. Suppuration took place, and the matter not being confined as in phlegmonous inflammation, extensive sinuses were formed, through which a probe could be passed in all directions. The disease was no sooner checked in the arm, than it invaded the leg and threatened to extend up to the body. The decoc. canthar. was again successfully applied—the leg now began to exhibit symptoms of amendment, but the arm remained for a long time in a dubious state, the cellular tissue having sloughed away, and the integuments being exteriorly detached from the subjacent muscles. These varied attacks of disease occupied about two weeks. The patient became greatly emaciated—there was a complete shedding of the cuticle, even of the tongue, which became smooth and polished, being divested of its papillæ. After being reduced to the verge of the grave, he finally began to show symptoms of amendment. About this critical period of his disease, buboes appeared in his groin and his neck, and in other glandular parts of the body, which suppurated and discharged great quantities of matter. From all these complicated ills, however, this poor man at length recovered, and when I last saw him he enjoyed better health than he had done for many years past.

The following cases are communicated by my friend and partner, Dr. FITHIAN, of this village. I give them in his own words.

“On the first of December, 1824, I was called early in the morning to Mrs. Cattell, aged about 43, of a robust constitution, whose early life had been active and laborious, but who

being now in easier circumstances, had become fleshy and rather corpulent. The history of the case, as I had it from the family, (for the patient could not speak intelligibly in consequence of a tumour in her mouth, hereafter to be described,) was that upon rising early in the morning she felt chilly, and in about fifteen minutes, a lump formed under her tongue, which increased so rapidly that by the time I arrived, which was at an early hour of the morning, it had attained the size of a small kidney, which it resembled very much both in form and colour. Upon a closer examination, I found the tongue thrust back, and that the tumour was formed by a great enlargement of the sublingual glands. It felt hard, but was covered with a thin membranous envelop, which being punctured discharged bloody water. She complained likewise of some soreness of the throat, but her pulse was quite natural, and as there were no other symptoms demanding particular attention, and as she had taken a dose of sulph. magnes. previous to my arrival, I contented myself with prescribing the following gargle:

R. sulph. cupri ℥j. to be dissolved in a strong infus. cinchon. ℥viij.—with directions that she should frequently wash her mouth and throat with it.

Dec. 2d. The tumour under the tongue had begun to cast off its membranous covering and looked less red; it had also somewhat diminished in size; she complained of nothing more. The salts had operated well the day before, and all appeared well.

Dec. 3. The tumour was now quite shrivelled, and had an appearance somewhat resembling a dried fig; she could now talk, and expressed herself much better; said she began to want food, and had eaten freely. Thinking that nothing more was now wanting but time and care to complete the cure, I dismissed my patient.

In the following night, however, I was again sent for, and on my arrival found that a new train of symptoms had occurred. The first expression from the patient on my entering the room, was "what shall I do? the pain in my leg is so violent it will kill me if I do not soon get ease." While

warming myself, I learned that in the course of the night she was suddenly seized with a pain in the calf of her leg which soon became very distressing, and from which she could get no relief. Upon examining the part I could perceive no discolouration or tumefaction externally, but upon grasping the calf of the leg I discovered in the lower part of the gastrocnemius muscles, a deep-seated tumour, which felt precisely like the mammary gland. I ordered a large blister to be applied to the calf of the leg, and opium to be given to ease the pain.

Dec. 4th. Saw the patient again about 10 A. M. and to my great astonishment, discovered the whole of the lower part of the limb to be in a gangrenous state, with considerable tumefaction, and vesicles in different parts filled with bloody serum. Upon removing the blister plaster, found it had drawn well, but the skin under it presented a dark red appearance. I now ordered a blister applied above the knee, and the free exhibition of internal stimulants. The pulse, during all this time, did not appear to indicate any particular derangement of the circulatory system. The pain, however, still continued. By afternoon the discolouration had reached the body, and streaks extended to the ribs. The skin under the throat had assumed a dark colour, and about midnight she expired—in less than twenty-four hours from the commencement of the tumour in the leg.

In the course of a few days afterwards, I was called to another case in all respects similar in its commencement to the preceding, except that the tumour under the tongue was not of so deep a red colour, for that in the case just related appeared like a bag of blood. The subject of this case was a Mrs. Ward, who lived on Timber Creek, and was aged about thirty-eight years, of a delicate constitution and spare habit. At the time I first saw her she had been sick two or three days; and the history she gave me was nearly the same as that of the preceding case. The tumour at this time had put on the shrivelled or fig-like appearance. Her pulse was not excited, nor did it indicate debility.

Feeling some alarm from the unfortunate issue of the case

I had just witnessed, and to which this bore so strong a resemblance, I immediately directed the exhibition of Madeira wine and the best red bark, in quantities as large as the stomach could bear, to be repeated every hour, and a gargle for the throat similar to that used in the former case.

That night great pain came on in the extremities, but a dose of laudanum, which I directed to be given in this event, gave relief and prevented a message to me in the night, which they were on the eve of sending.

The bark and wine were continued a few days until convalescence was complete. The patient was dismissed cured."

A case precisely similar to the two preceding occurred about the same time to Dr. GARRISON, a respectable practitioner, who resides at Swedesborough, about ten miles from this place. This case terminated fatally.

The circumstance of so many of these anomalous cases of a similar character, occurring in the same district of country, and in the same neighbourhood, naturally leads to the conclusion that they must have proceeded from some common cause, peculiar to that section of country. What this may have been we can only conjecture. There was nothing peculiar in the locality here that did not exist in the adjoining neighbourhood—nothing in the state of the atmosphere or of the weather, as far as we know, that did not equally exist throughout an extensive range of similar country. It may, perhaps, afford some clue to aid the speculations of the theorist, to be informed that the bread corn chiefly raised in this district is rye, and that the families in which these cases occurred, were in the habit of using this kind of bread exclusively. This grain, it is well known, in certain seasons, is liable to degenerate into a fungous or diseased growth, called ergot, (*secale cornutum*), which is well ascertained to be pernicious to health, and in certain parts of France, where this grain is principally consumed, it has given rise to a disease somewhat analogous to the one in question, called dry gangrene. A species of gangrene, affecting the feet and legs of cattle in this and other districts within a few years past, has been satisfactorily traced to an ergot substance, produced by the common meadow

green or spear grass, (*poa viridis*.) That the rye used by the families thus afflicted, was more contaminated with the ergot than usual, I have not been able satisfactorily to ascertain, but that it contained more or less of this substance, there can be no question.

As I have spoken of the decoc. canthar. in sp. terebinth. among the remedies used in the preceding cases, it may not be amiss here to remark, that I have frequently had occasion of late to use this preparation in cases of erysipelatous inflammation, and that its efficacy, as a local application, in checking this formidable disease, is truly surprising. For a knowledge of this fact, I am indebted to my ingenious friend Dr. J. REEA BARTON, who is entitled to the credit of its discovery. He suggested its use to me while in the Pennsylvania Hospital, in the summer of 1824, where I had frequent opportunities of trying this preparation as a remedy for erysipelas, and in every instance *there*, with the most happy result. The best mode of application is to wet the patent lint or soft old muslin, thoroughly with this decoction diluted with sweet oil, in proportion to the strength of the former, (which is a matter of great importance to be attended to,) where it is of full strength, or what is called a saturated decoction, I use equal parts. The lint or muslin is to be bound on the inflamed surface, taking the precaution to cover it with oiled paper, to prevent the absorption of the liniment by the bandages or cloathing. The application may, if necessary, be renewed, in from four to six hours, but I have frequently found a single dressing sufficient to check the disease. Stimulating and irritating as this preparation might be presumed to be, if its strength is properly graduated it immediately soothes and allays all pain, and I have known patients who suffered agony under the use of simple emollient poultices, to find almost instantaneous relief on the application of this preparation, being analogous in this respect to the stimulating remedies now so generally used in burns. The best and most simple rule for graduating its strength, is to raise it just to that point at which it will pucker and

slightly elevate the cuticle, without actually producing vesication.

Amongst the cases wherein I have more recently had occasion to use this remedy, were several in which the face and scalp were involved, and in no one, that I now recollect, where it was thoroughly applied, has it failed to arrest the disease in twenty-four hours. It is most effectual in those cases where the disease is of the more superficial kind; and in those of mere erythema a single application will generally suffice. Blisters, in parts too weak to bear excessive inflammatory action, will sometimes produce mortification, as is exemplified in cases of old people, and sometimes in erysipelatous inflammation. It was an instance of this latter kind which first lead Dr. Barton to try the remedy in question, as a milder means of overcoming the morbid action, wisely believing that the specific effect of the flies might, in this mode, be produced without incurring the risk to be apprehended from blisters.

I had hoped that Dr. Barton would, ere this, have given publicity to his discovery of this remedy for erysipelas, the importance of which certainly entitles it to be extensively known, and I trust he will pardon me for forestalling him, in having thus incidentally mentioned its usefulness.

## REVIEW.

ART. XIV. *An Essay on Conception and Superfætation.* By JOHN STEARNS, M. D. of New York, late President of the State Medical Society. Read before the Physico-Medical Society, 1825.

TO the term generation very different meanings are attached in the several sciences, which comprise the great circle of philosophy, or human knowledge. By it, however, we understand that process of the animal economy by which the species is propagated, and it is in this limited sense we are now to discuss it. Generation is peculiar to the living state. The change which take place in dead matter, such as its decay and reproduction, are effected by laws very contrary to it, in their nature and mode of operation.

By Providence it is ordained, that all animated and organized bodies shall fall by the indiscriminate hand of death. But the period of life is exceedingly diversified in the different classes. There are some of these beings whose existence may be protracted for a century, while others can only endure for a few years, months, days, or hours. Even man, with all his superior attributes and faculties, is subject to the same inexorable law. The victim of an inevitable destiny, his body too, dies and rots, like that of the meanest reptile which crawls the earth; exempt from this dismal fate, it is his soul alone, which partakes of the immortality of his God.

But, though individuals perish, the race is preserved from extinction. Thinned by the ravages of time, the ranks of existence are instantly filled by new creations. Deaths and births alternate in a steady and regular order. The moment that terminates the vital course of some, brings others to its commencement, and never, says a poet of antiquity, does morning or night visit the globe without witnessing funeral

lamentations around a grave, or the plaintive cries of an infant in the cradle.

Generation, is confessedly the greatest mystery in the economy of living beings. Curious in every view, though more especially in relation to the origin of ourselves, it early engaged attention, and has since been investigated with no diminution of ardour, or deficiency of ingenuity. Notwithstanding, however, the labour which has been lavished on the inquiry during this long succession of time, we have still to regret the obscurity in which the subject is involved.

The result, indeed, of all this diligence of research, is little more than the determination of a few facts. But, as is always the case, where the subject is imperfectly understood, an infinite number of conflicting hypotheses, almost equally weak and visionary, have been constructed out of these defective materials, and promulgated, sometimes, in a tone sufficiently confident and presumptuous.

Considering the variety which prevails in the figure, the structure, the economy of the numerous creatures constituting the vast and lengthened chain of animated nature, it may be readily imagined, that the generative process must be singularly diversified, and in many instances widely discrepant. To trace all these varieties, however interesting the review might be, would occupy more time than we can spare, and could hardly fail to be deemed an improper encroachment on the province of natural history. It is our design, therefore, to restrict the ensuing inquiry to the function in our own species, referring occasionally to it, as it is modified in the inferior animals and vegetables, merely to borrow, for the purpose of illustration, some of those striking analogies which they afford. That the ovaries are the seat of conception, is one of the few points, connected with this difficult subject, which seems to be indisputably settled. Each of these organs has near its surface a series of vesicles, holding, as in a reservoir, a small portion of pellucid fluid. Now, it has, of late, been distinctly shown by experiments, that after a fruitful coition, one or more of these vesicles become changed. The

alteration to which we allude, consists in a gradual enlargement of them, together with a loss of transparency in the contained fluid, its assuming an opaque, reddish hue. It further appears, that the impregnated vesicle, after passing through the several stages of maturation, somewhat in the manner of a small abscess, finally bursts, and discharges its contents, which being laid hold of by the fimbriated extremity of the fallopian tube, are conveyed to the cavity of the uterus to be evolved and perfected into a fœtus. The cavity in the vesicle is gradually filled up by a deposition of coagulable lymph, forming a small indurated body, of a yellowish aspect, denominated corpus luteum, which appearances correspond exactly with the number of conceptions, and are now commonly held as criteria of that process having taken place.\* But, independently of the evidence thus afforded of the ovaries being the seat of conception, we have the conclusive fact of the fœtal rudiments having been detected in the fallopian tube, on their passage to the womb, in several of the inferior animals, and even in the human species cases of extra-uterine conception are of frequent occurrence, where the fœtus, completely formed, is found attached to some of the abdominal viscera, or retained in the tube, or the ovary itself! Nor is it less certain, that the phenomena exhibited in the ovary, constituting the proximate steps of conception, are owing to the fecundating influence of the semen masculinum. This point is as clearly proved as the former, and as universally conceded. But no slender difference of opinion exists among physiologists, as to the precise mode in which the seminal impulse is imparted.

It is by many maintained, that the semen being deposited in the uterus, is speedily taken up, and without having suffered any material alteration in its properties, conveyed directly to the ovary. In the early season of physiological

\* It has, within a few years, been maintained by Sir Everard Home, in opposition to the above views, that the fœtal rudiments are secreted by the corpora lutea, without, however, in our opinion, sufficient plausibility to entitle the hypothesis to attention.

science, such an hypothesis was respectable, and will always seem sufficiently plausible to those who are content to receive impressions from hasty and imperfect examinations.

The objections which we have to offer to it will be comprised in as small a compass as possible. We shall endeavour first to show, that the semen does not at all enter the uterine cavity, and much less, that it reaches the ovary.

Those who differ from us on this subject, have mostly insisted that the semen is injected into the uterus by the penis. It is true that some other modes have been suggested. Thus, it was taught by no less a character than ARISTOTLE, that the uterus during the congress of the sexes, snuffs up a fecundating halitus from the semen, as air is inhaled by the nostrils. Not to be outdone, PLATO also advances a speculation, in which, comparing the uterus to an hungry animal, he supposes the one, to suck in the semen with the same avidity as the other snaps at food, congenial to its taste. Exceeding all, however, in absurdity, is the notion of a modern writer who gravely surmises, that the os tincæ in the act of coition is drawn over the glans penis, pretty much, as we put on a night-cap !

Dismissing, without comment, these crude sallies of the imagination, let us recur to the first hypothesis, which is pressed upon us not less by its seeming intrinsic probability, than by the weight of the authority from which it has and continues to derive support.

That the male organ is endowed with considerable projectile power is too conspicuously evinced by the impetus with which the urine is discharged, to be denied. Engaged, however, in the act of coition, its capacity in this respect, is greatly diminished or wholly suspended by the firmness with which it is grasped. The peculiar nature of the semen is an additional cause of resistance, it being plain that so heavy and glutinous a fluid cannot be thrown to any distance. But admitting, that by an unusually vigorous impulse, the semen were projected as far as the uterus, how can it enter the cavity of that viscus ? It is to be recollected, that the os tincæ, at least,

in the virgin state, is nearly as small as the opening of the urethra in the male, and instead of being placed in the immediate axis of the vagina, inclines more or less to the one or other side, or towards the sacrum, so that the apertures of the two organs are not in opposition. Besides this, the *os tinæ* is, for the most part, filled with a thick glutinous matter, and where it is wanting, as is generally the case, in the virgin uterus, the hard unyielding lips of the *tinæ* are so closely approximated as to be nearly closed.

Nor are these the only obstacles to the passage of the semen. The canal leading through the neck and body of the uterus is, in the unimpregnated state of the organ, probably not much larger than a common size probe. That portion of the canal called the *straight* is still more contracted, and along the whole of its course, there are *striæ* or wrinkles, and between which glands exist, secreting mucus calculated for the purpose of further obstruction. Even the proper cavity of the uterus itself, is so extremely shallow, that its two surfaces are nearly in contact.

All that we have hitherto stated relates to such impediments as are incident to a perfectly natural and healthy condition of the parts. To these, may be added others, resulting from morbid derangements, or congenital deformities, as well in the male as in the female organs.

1. The penis has its power of ejecting the semen, destroyed or abridged by truncation, by strictures, by anomalous openings along the urethra, or by debility and relaxation.

2. The vagina may be obstructed, or shut up, by cohesion of its sides, by membranes of adventitious growth, or by tumours.

3. The *os tinæ* is sometimes met with impervious, either from original imperfection, or by the process of inflammation, and is occasionally rendered utterly inaccessible to the semen by the obliquities, retroversions, or prolapsions of the womb.

Each of the preceding positions is amply supported by cases to be found in the writings of HARVEY, MORGAGNI, HILDANUS, RUYSCH, MAURICEAU, SIMPSON, GUILLEMEAU, HALLER, and in the several periodical publications. Not

the least curious case of this nature is recorded by Dr. CHAMPION. On visiting a woman, who had been three days in her first labour, he found the orifice of the vagina obstructed by a firm and very thick membrane, except in two points so small, that the largest aperture would hardly admit a stylet of the size of a pin's head. The urethra was so much dilated as freely to admit the fore finger to pass into the bladder. On dividing the obstructing membrane, the vagina was found of the ordinary dimensions, and the woman happily delivered of twins. By subsequent inquiries, it was clearly ascertained, that the membrane had previously existed to her marriage, as she was subject to vast accumulations of the menstrual fluid, which came away very slowly through the two minute openings. It further appeared, that the husband, unable to rupture the membrane, had so dilated the urethra, as to substitute it in his embraces for the vagina, which, however, he did not accomplish till the fifth month of pregnancy, having prior to this time, never penetrated beyond the vulva.\*

These facts very clearly demonstrate, that conception can take place, though the semen may be deposited merely within the vulva, and seem almost to warrant the conclusion that it never does, as a natural event, reach the cavity of the uterus. Lest, however, they may not appear to others in the same strong light in which they present themselves to us, we shall bring to their support some auxiliary evidence.

Experiments have been instituted to decide this point, by HARVEY, DE GRAAF, LEWENHOECK, HALLER, HAIGHTON, on different animals. The doe, the cow, the ass, the ewe, the bitch, the rabbit, were all inspected immediately, or at remoter periods, after connection with the male, and never, except in one instance, could the semen be traced beyond the vagina. By Haller it is stated, that he once detected it in the uterus of a sheep forty-five minutes post coitum. But this is a solitary exception to the numerous observations of himself and others, and which can claim little consideration, especially

\* London Med. and Phys. Journal, for July, 1819, p. 27.

when it is known, that such a result was especially necessary to the maintenance of a favourite hypothesis. It is true, we have floating about a story, of the late Mr. HUNTER having also discovered semen in the uterus, in an experiment which was made on a bitch in coitu. But it is no where recorded, and being vaguely preserved by tradition only, has never been much insisted on in the present controversy. Had so decisive an experiment been really made by Mr. Hunter, why was it withheld from the public? As many of his physiological researches were characterized by equal cruelty, he could not have been restrained by any apprehension of incurring reproaches on this score. Even, however, admitting that the experiment was made, which we altogether doubt, since we have never known an allusion to it, either in the oral discussions of the debating societies of London, which we attended, or in the controversial writings on the subject, we are sure that he was betrayed into error.

It is now well ascertained that the emission of the male dog takes place prior to his being fastened to the female. As soon as this happens, the *trist* state succeeds as is denoted by every appearance, and hence the injection of the semen could not have been seen. Either, therefore, Mr. Hunter was deceived, or he has been inaccurately reported.

In aid, however, of the single experiment of HALLER which we have just noticed, it is urged with some degree of confidence that MORGAGNI saw the semen in the uterus, and RUYSCH in the fallopian tube, in the human species. Without impeaching the veracity of either of these illustrious men, we may be permitted to remark, that their observations have never been confirmed, and, that under the circumstances in which they were made, it is reduced almost to a moral certainty, that they mistook for semen, what was in reality the mucus of the parts. Conceding, however, to these alleged facts all that can be reasonably required, and what do they amount to? Contrasted with the vast mass of counter evidence, they dwindle into insignificance, and will not weigh as dust in the scale.

As it appears, therefore, that the semen does not enter the

uterus, it becomes superfluous to inquire respecting the practicability of its conveyance by the fallopian tubes. The latter problem is merged in the former. But to silence all cavils, we shall give to the question a cursory examination.

That the fallopian tubes are not subservient to this purpose, is very distinctly indicated by the peculiarity of their structure. Commencing with an aperture so minute as hardly to admit a common bristle, the canal gradually enlarges, and finally terminates in a wide and patulous mouth. Now, were they destined to convey *from*, instead of *to*, the uterus, would not the construction be directly the reverse of what it is? It is known that they conduct the product of the ovary to the womb, and we see that the extremity is adapted to this office. But conveying nothing from the uterus, the orifice is fashioned accordingly. By assigning to the tube this complex function, we, moreover, invest it with the power of a two-fold action, diametrically opposite, of which there is no analogy in the animal economy. The inverted peristaltic motion of the intestines, comes nearest to an example, though it will not hold. The parallel is imperfect, inasmuch, as the action of the intestine is preternatural, the effect of violence or disease. It is useless, however, to protract this discussion, as we have proof at hand, which is absolutely conclusive. By the experiments of HAIGHTON it is ascertained, that the tubes do not change their position to grasp the matured vesicles, till the whole process of conception is consummated in the ovary. This eminent physiologist concludes, "from a series of observations made on different rabbits, at every hour between the first and the ninth, that the fimbriæ remain nearly in their usual situation, and the only difference he found in the last hour was a greater turgescency of vessels, as if preparatory to some important action. On the ninth hour he desisted from the inquiry, because, says he, "the ovaries have evident marks of impregnation, and there appeared to be no action in the tubes, by which the semen could be conveyed to them."

Convinced that the hypothesis in its primitive shape was no longer tenable, some of the advocates of impregnation by

contact, have contended that it is effected by the emission of a subtle exhalation from the semen, termed *aura seminalis*, and which is transmitted through the tubes to the ovary. But here they are again met by the whole body of facts, and chain of reasoning, which drove them from their original position. It has indeed been said, and with no want of plausibility, that the volatile vapour from the semen might penetrate through obstructions which would resist the semen itself. Conceding that in some instances it might happen, how can the cases formerly referred to be got over, where, from organic derangement, or as in the experiments of Haighton, the passages were so utterly occluded, as to be impervious even to air?

Nor are these the only difficulties which stand in the way of this amended hypothesis. But we are not disposed, at present, to enter into any details. Before engaging in a minute or lengthened investigation of this sort, we shall require it to be shown, that the *aura seminalis* has in any degree the property of fecundation. As yet no such proofs have been exhibited. The experiments of SPALLANZANI and HUNTER, the only ones which have been made on the subject, prove, indeed, quite the contrary.

Distrusting also the original hypothesis, another set of seceders from it have resorted to the general circulation as a channel through which the semen is conducted to the ovary. There are two branches of this latter doctrine, differing, however, so slightly as not to deserve to be particularized. Each is liable to the same objections. No one has shown, that the semen, diffused in the mass of blood, retains any of its powers, or, if it does, why it should be specifically determined to the ovaries in preference to any other part of the body? Is it indeed to be credited, that so small a portion of semen, when diluted in twenty-five or thirty pounds of blood, should be competent to the end alleged?

Much force has been attempted to be given to the declarations of Spallanzani, that he impregnated the ova of frogs with semen not less diluted. But subsequent inquiries have discovered, that the seminal liquor is not miscible or soluble

in water. Of this fact, any one may convince himself, by inspecting a pond of water, in which frogs exist, during the copulative season of these animals. The ova of the female, as well as the sperm of the male, will be seen floating on the surface, without the least tendency to commingle. This indeed, is so little the case, that there are some species of fish, and other aquatic animals, which effect the scheme of generation by throwing the ova and semen into the water, leaving them to come in contact by winds, currents, or any other extraneous or accidental agency.

After what we have said, it is reasonable to conjecture, that if Spallanzani did really fecundate the ova of frogs, as is averred, it was accomplished by entangling with the pencil or small brush employed on the occasion, the seminal particles swimming on the surface.

What, however, is the analogy between simple solution, or mixture, allowing the truth of the experiments, and those changes wrought in the properties of substances by the combined operation of digestion and assimilation? To believe indeed, that the semen retains its efficient powers after traveling through the circulation, and having performed this protracted voyage of circumnavigation, would be a stretch of credulity wholly at variance with the dictates of reason, the suggestions of experience, and the lights of analogy. But this is a point put beyond the reach of cavil or disputation. Experiments carefully made, and often repeated, prove to absolute demonstration, the entire conversion, so far as regards its activity, which every substance undergoes, preparatory to its admission into the circulation. In making this averment, we are not ignorant that some of the properties of certain matters may be traced in the secretions and excretions, such as the odour of garlic, the colouring matter of madder, &c. But it is manifest, that the active properties of these articles are previously destroyed or neutralized by the process of animalization, and nothing remains saving the nutritive, or, at least, inoperative parts. In this conclusion, we are warranted by a variety of considerations distinct from those already stated, and especially by the incontrovertible

fact, that, however bland the fluid may be, whether milk, or mucilage, or pus, or oil, it cannot pass, even in the smallest quantity, directly into the blood without producing the most fatal consequences.

The advocates of this hypothesis are, perhaps, not aware of the ridiculous extent to which it leads. It follows, as a necessary corollary from their premises, that the business of generation may be carried on by merely inoculating the female with semen, and thus we might propagate children with as much facility as a venereal bubo is excited, or, as we raise a crop of variolous pustules.

It has indeed been maintained, and by no less a person than the illustrious Harvey, that the work of generation really goes on somewhat in this way. Denying that the semen enters the uterus, he thought that this organ conceives by a sort of contagious action communicated to it by the seminal impulse—as the magnet imparts its virtue to steel.\*

In closing the examination of the hypothesis which we have so strenuously combated, we must repeat, that we deem it utterly unfounded. Not less absurd or unsupported, in our estimation, was the notion of a professor,† who taught, that in the sexual congress of frogs, the semen passes from the toes of the male through the axillæ of the female into her genital system, and that, in the same process, among pigeons and sparrows, the fluid is vomited by the male, into the mouth of the female. Let not our readers be startled at such preposterous speculations, or imagine that they are the offspring of our science in the season of its infancy and feebleness! Every day presents us with similar vagaries, which are formally inculcated and eagerly adopted. Even the vigorous mind of LINNÆUS, illuminated by the full blaze of science, was so far led astray, when engaged in speculating on this subject, as to advance the vulgar opinion, that impregnation takes place in fish, by the female pursuing the male, and drinking in the seminal liquor, which he emits in the chase.‡

\* Buffon, p. 21.

† Menzies, vid. Spall. Diss. p. 11. et p. 103. vol. 2.

‡ Spall. Diss. p. 103.

Conceits like these might indeed provoke a smile were they harmless, or if their consequences only attached to those with whom they originate. As this, however, is not so, they cannot be too severely censured, or warmly resisted. More than any other cause, they degrade genius, bring learning into disrepute, and expose the science we particularly cultivate, to the sarcasms of the witty, and to the just, though contumelious reproaches of the sober, the circumspect, and the wise.

We proceed with the inquiry relative to the mode of fecundation. It has of late been conjectured, that the semen may be applied by means of absorbents, communicating between the vagina and ovary, designed exclusively for this office. Before such a deduction can be entertained, surely these absorbents ought to be demonstrated, or some better proof of their existence afforded. Neither has hitherto been done. There is, on the contrary, the strongest presumption, that none such exist. The lymphatics of the vagina have been as distinctly traced as those of any other portion of the body, and consist of two sets, the one terminating in the sacral, and the other in the inguinal glands. Not a twig has been perceived in the direction of the ovary.\*

Nor would the existence alone, of a set of absorbents be all that a cautious inquirer ought to demand to verify the conjecture. It must also be proved, that these vessels do not, like the rest of the lymphatics, digest and animalize whatever they take up, or the semen would be so altered, in the progress to its final destination, as to be entirely inefficient. That the absorbents are endowed with the power which we have here ascribed to them, is too strikingly evinced to be denied. It is one of the provisions of nature to prevent noxious substances from penetrating into the circulation unchanged. In most instances they are fully adequate to this end, and where they are not, the first conglobate gland takes on inflammation, and thereby intercepts the further progress of the poisonous

\* We have lately seen a vague notice of a supposed discovery, by a Danish physiologist, of a set of lymphatics, running from the vagina to the ovaries in the sow, which, however, remains without confirmation.

matter. Considered in this light, these glands may be regarded as centinels guarding the exterior approaches of the body from danger, which might otherwise assail it.

As yet, therefore, the hypothesis under review is purely gratuitous. It is built on a postulate, of which there is not the slightest evidence, and results from the most wretched species of philosophizing. Give me, cried out ARCHIMEDES, with the enthusiasm of genius, a spot on which to fix my machinery, and I will move the globe. Equally may the theorist exclaim, in the ardor of confidence, grant me my premises, and there are none of the arcana or mysteries of nature, which I will not reveal and explain. Nothing is more easy. To answer such purposes, hypotheses may be made to start up like exhalations. Be it remembered, however, that these intellectual phantasies have ever proved the bane of truth and the curse of medicine.

It results, we think, pretty satisfactorily, from the preceding investigation, that the semen is neither applied directly to the ovaries by the fallopian tubes, nor more indirectly in the shape of *aura seminalis*: not through the general circulation, nor by any specific set of absorbents.

How then is the important end of fecundation accomplished? We answer, by that law of the animal economy termed sympathy or consent of parts. Let not our readers be surprised at this declaration. When the theory comes to be developed, it will appear to have much stronger claims to regard than any which has been advanced.

To Dr. Haighton, an experimental physiologist, who is not less distinguished by vigour and sobriety of judgment, than by the vivacity and alertness of his genius, we owe this fine specimen of inductive philosophy. Extraordinary as it may seem, we repeat, that this has a decided superiority over every rival hypothesis. It is, as all legitimate theories must be, regularly deduced from well established facts, and we shall presently see, that it accurately comports with the phenomena of conception, with the particular changes which take place in the uterine system, with some very striking analogies, and with the settled laws of the animal economy.

As was to be anticipated, it has nevertheless not escaped opposition. No medical theory, can, perhaps, in the present state of our knowledge, be so perfectly constructed as to defy attack. The penetration of an ingenious adversary will expose what is really, or is made to appear, some lurking error or defect in its composition.

Two objections have commonly been preferred against the theory before us. It is alleged, in the first place, to be utterly contradicted by analogy. The experiments of SWAMMERDAM, ROEZEL, and SPALLANZANI, are said to prove very conclusively, that the ova of certain amphibiae are fecundated, by the direct application of the seminal fluid, as they pass from the female. Every one will admit the fact, and even more, that the process can be artificially done,\* to which, however, it may be replied that we cannot be too slow or reluctant in receiving inferences of this sort. It has been justly remarked, that analogies serve at all times much better for the purpose of illustration than of argument, and should be appealed to in support of our reasonings with no little circumspection. Before they are adopted, or are entitled to the slightest weight, it must be ascertained that they are close and pertinent in every respect.

The analogy adduced on this occasion is certainly very remote and defective. The cases, indeed, are entirely dissimilar. In the animals to which we have alluded, impregnation takes place out of the body, and nature could have pursued no other course, than the one she has chosen, to effect this end. But it is urged also, that impregnation in vegetables is conducted in the same mode. It may be so, though there are great doubts of the fact. By LOGAN, our countryman, the real author of the discovery of the sexual system, it was utterly denied, having been led to this conclusion by the contemplation of the extreme complexity of the genital apparatus. As regards some particular plants, it is conceded universally, that their organs are so contrived, that the pollen, or fecundating powder, cannot possibly penetrate to the ovary,

\* Experiments of Spallanzani.

and consequently, in such instances, conception must be accomplished by means somewhat similar to the one for which we are contending.\*

It seems to us, that analogical arguments afforded by the generative process in birds, deserve infinitely more consideration than those derived from either of the preceding sources of comparison, and especially from the amphibious animals. Conception here takes place within the body, and the structure of the parts subservient to it, is not so unlike the genital organization of the human subject, and other mammalia.

In birds, the ovaries are situated high up the spine. They have the infundibulum, which corresponds to the fallopian tube. The uterus is long and convoluted like an intestine. Now, in the act of copulation, though the male is without a *penis intrans* or the power of projection, and deposits the seminal liquor merely within the vulva, at a single connection—all the existing ova, amounting commonly to twenty or upwards, will become fecundated. This interesting fact, which was originally noticed by HARVEY, has since been fully confirmed by the observations of SPALLANZANI and of CLINE. Can it be credited, that so extensive an impregnation is produced by the contact of the semen? Be it recollected how long and tortuous is the uterus, how remote the ovary, and how difficult of access.

But though the force of this reasoning is admitted, it is still objected to our theory, that it is impossible to comprehend in what way so singular a process as that of conception, can be carried on by a mere sympathetic agency. None of the phenomena of the case, it is alleged, are at all explicable by it. Can we, it is exclaimed, account for any one of the proximate or successive steps of the process, on this principle? How explain the fecundation of the ovary? How the resemblance of children to their parents? How the peculiarities of hybridous productions? How the transmission of hereditary diseases?

In any and every view which we may take of these questions,

\* Vide a paper on this subject by Mr. Nuttall, in this Journal, vol. vii.

it must be confessed, that we shall find them full of difficulty and obscurity. But are these increased by the adoption of the doctrine of sympathy? Do the ancient hypotheses, founded on the presumption of the direct application of the semen, unravel our perplexities, or dissipate any of our doubts? Why, if they afford us this aid, are we not content with them, and cease to inquire after new solutions? The truth, however, is, they lend no one ray of light, or a single thread of a clue to guide us in the darkness and intricacy of the labyrinth.

Whatever defects, therefore, appertain to the sympathetic theory, may be equally traced to all others. The points which we cannot satisfactorily explain, at present, are mysteries that will probably continue as heretofore, to elude our researches, and may never be revealed. It is very obvious to us that all the difficulties in the way of the adoption of our theory proceed from the prejudices of the humoral pathology.

Of this antiquated and truly absurd system, we still retain much, even at the present day, insinuating itself into our reasonings and opinions, and clouding many of our medical views.

By the humoral pathology it was held, that all substances, by whatever avenue brought into the body, are conducted through the circulation to some part on which they make an impression. Every article, whether nocive or medicinal, was supposed to produce its effects in this way. No other idea, in short, was entertained of the operation of agents on the living frame, except by direct touch, or mixture, on some portion of the solids, or with the fluids.

Now, perhaps, we may render our theory acceptable, even to the humoral pathologist, by showing that a sympathetic action is precisely the same as that produced in either of the above modes. As preliminary to the main consideration, we assume it as indisputable, that whenever any agent, remedial or poisonous, is applied to a susceptible portion of the body, externally or internally, an action is excited which is extended, more or less according to the diffusibility of the article, or the degree of sympathetic connection which the part may have with the body generally. Thus a set of actions

is raised, every one of which is precisely similar, provided they are confined to the same system, by which it is to be understood, parts of an identity of structure, or having the same official capacities. But where the chain runs into other systems, it loses its homogeneous character, the actions being modified according to the peculiar organization of the parts in which they may take place.

To illustrate more distinctly our meaning, we will state a very familiar case. By inserting a particle of varicellous matter, under the skin, local irritation is created. In a few days this action becomes diffused, and a fever ensues, which, after a short continuance throws out an eruption, each pustule of which is alike, because the surface of the body is of a uniform structure, containing exactly the same sort of virus as the primary or parent pustule. It is, in this way, that morbid action distributes itself. When diseases arise from a point, as in fact all diseases do, though more strikingly those occasioned by inoculation, the matter introduced is not infinitely divided and spread over the body, but the action which that matter had originally excited. These are general principles, which apply to the system in every condition, and explain the *modus operandi* of medicines, as well as of the causes of disease. Whatever, in short, operates on the living frame, is obedient to the same laws. The spot first acted upon is the focus from which is irradiated the more diffused impressions.

By embracing these views, we are supplied with a very rational solution of the mode of action of the semen. The uterine system consists of the matrix, the tubes, the ovaries, and vagina. It is a whole, made up of parts, between which the closest and most intimate relations prevail, in consequence of reciprocal sympathies. Next we will carefully trace in regular progression, the phenomena of conception, in order to determine how far they are explicable on the principle we have stated.

Deposited in the vagina, the semen commences its stimulating operation. The impression thus made is communicated to the uterus, and finally to the ovaries, the effect of which

is, one or more vesicles enlarge, project, burst, and discharge their contents. During this process, the tube is undergoing a state of preparation for the purpose of embracing the ovary, and receiving the rudiments of the foetus. This preparation consists, in part, of an increased turgescence of its vessels, and a consequent enlargement of its fimbriated extremity. Being prepared, it slowly rises out of the cavity of the pelvis, and approaches the ovary.

After the tube has performed its office of conveying by a peristaltic motion, the contents of the vesicles to the uterine cavity, it gradually returns to its former situation and condition. But, while these different actions are going on in the appendages, others not less important to the main design, are instituted in the uterus itself. It is engaged in the fabrication of the *membrana decidua* to afford a medium of attachment to the delicate ovum, till the placenta is formed, and to guard more effectually against its escape out of the uterine cavity, the *os tincæ* is sealed up by a deposition of a thick tenacious mucus.

Nor does the care of nature stop here. As the foetus advances towards maturity, a provision is making for its future nourishment, by calling into exercise the secretory function of the maternal breasts. Every part of the preceding account is so well authenticated by experiments, or well known facts, as to admit of little doubt or disputation. Tracing these actions, one by one, beginning at the vagina, then proceeding in succession to the ovary, to the fallopian tubes, to the uterus, and the breasts, we cannot resist the conviction that they are a series of links of a great and consecutive chain of association, and, therefore, that conception results entirely from that leading law of the animal economy termed sympathy, or consent of parts.

That a portion, at least, of these operations, is of the nature for which we contend, cannot be disputed. Every one must concede, that the gradual enlargement of the *mammæ* which commences soon after conception, and the secretion of milk that ultimately takes place, are owing to a sympathetic impulse received from the uterus. Why, then, if two organs,

so remotely separated, can be thus brought into consent, and made, in this manner, to perform their respective functions, may not the several parts of the uterine system be similarly affected? why, in short, may not the impression primarily made by the semen on the vagina be extended to the ovary? Let what we have said be well considered, and our readers can hardly fail to give their assent to this doctrine. We wish them to bear in mind, that the semen first stimulates the vagina, and that by sympathy, the ovarian vesicles enlarge, project, and burst—that by sympathy the tubes incline to the ovaries, and having embraced them, convey the rudiments of the fœtus to the uterus—that by sympathy the uterus makes the necessary preparations for perfecting the formation and growth of the fœtus: and, finally, by sympathy, the breasts furnish milk for its support after birth.

We have now gone over the first and most important part of the Essay before us, in which we have the misfortune to differ totally in all the views which it inculcates. Having appropriated as much space as we can at present allot to the subject, we must postpone the consideration of the second part to our next number. We cannot, however, take even this temporary leave of our author, without assuring him, that irreconcilable as our opinions may be, our feelings towards him are most respectful and friendly.

## QUARTERLY PERISCOPE.

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### EUROPEAN INTELLIGENCE.

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#### ANATOMY AND PHYSIOLOGY.

*General Remarks on the Aponeuroses, by P. N. GERDY, M. D. Paris.*—1st. In man, all the muscles of the body, trunk, and limbs, with the exception of the cutaneous, are enveloped by aponeurotic, fibro-cellular, and sometimes cellular membranes only, but which are exactly analogous to each other; thus the muscles of the anterior part of the neck, the larynx and trachea, the œsophagus, jugular vessels and nerves, are enveloped by the cervical aponeurosis, which passes under the platysma myoïdes and is attached to the sternum, clavicles, os hyoides, stylo-hyoid ligament, &c. &c. The submaxillary muscles are surrounded by a submental aponeurosis attached to the os hyoides, to the stylo-hyoid ligament, the lower jaw, &c. These aponeuroses are fibro-cellular only, throughout the greatest part of their extent. The muscles of the posterior part of the neck are covered by a fibro-cellular membrane, which is very strong between the superior extremities of the trepezius and sternomastoid tendon, with the occipital portion of which it is continuous. Below, this aponeurosis is lost in the cervical, around the lymphatic ganglions of the neck, in the same manner as the brachial fascia in the axilla. The pectoralis major is cloathed by a thick layer of cellular substance, attached to the clavicle and sternum, and this is continuous with the fibro-cellular envelopes of the muscles of the belly, of the side of the chest, of the axilla, and finally with the ligamento-cellular lamina covering the deltoid. The external oblique muscles are covered by se-

veral cellular membranes, one of which, somewhat more dense, has received the name of fascia superficialis, which is continued above and below, with the membranes of the lateral parts of the trunk. The serratus magnus, and the dorsal portion of the trapezius, are enveloped between the ribs and vertebral column, by membranes which are ordinarily cellular, but sometimes ligamento-cellular; these are attached to the spines of the vertebræ by tendinous fasciæ, as I have heretofore shown in my Researches on the Anatomy of the Regions. It is evident that these membranes are analogous to the fibro-subcutaneous layers of the abdomen with which they are continuous, and with which the superficial fascia is united. The sub-pelvic muscles are covered by a perineal aponeurosis, which encloses them between it and the internal pelvic aponeurosis. In relation to the muscles of the limbs, all know that they are enveloped throughout their extent by aponeurosis, but I add that these are covered by cellular or fibro-cellular membranes, which contain the subcutaneous nerves and vessels in their thickness. These membranes are found even to the extremities of the fingers, and are not devoid of strength even there.

2nd. All the aponeuroses of the muscles of the trunk placed within the aponeuroses and membranes of which we have spoken, serve for the attachment and prolongation, and always for envelopes to others. Thus the abdominal aponeurosis serves as an attachment to the three muscles of the belly, and for an envelope to the rectus abdominis and pyramidalis; which have each their peculiar bed, and this aponeurosis in being reflected from the crural arch in front of the *fascia transversalis*, envelopes the transversus abdominis and internal oblique. The lumbar aponeurosis, which is analogous to it on the posterior part of the trunk in receiving the fleshy fibres of the latissimus dorsi, the serratus inferior posticus, sometimes of the external, and uniformly those of the internal oblique and transversalis, incloses at the same time part of the common mass of the sacrolumbalis, longissimus dorsi, and multifidus spinæ, between two aponeurotic layers and the vertebral column, and in another part the quadratus lumborum in another sheath.

3d. The muscles of the limbs are almost all enveloped, each separately in a special sheath. There is but a very small number which are enclosed together in one sheath. The sheaths

have two, three, or four distinct sides, and are formed sometimes by perfect aponeurotic layers, sometimes by an aponeurotic or a cellular layer, as clearly *insulated* and *distinct* as the first mentioned, and by the bone to which they are all fixed. The intermediate layers of two neighbouring muscles concur in forming these sheaths, which, like partition walls, circumscribe and insulate them.

4th. The supposed tendons of many muscles concur in forming but membranous sheaths which form hollow pyramids, with three or four faces, the angles of whose internal surface separate the fleshy fibres to extend over the whole circumference of the tendon, which itself occupies the centre of the sheath. Such are the tendons of the flexor carpi radialis, palmaris longus, flexor carpi ulnaris, pronator radii teres, &c. &c. Moreover, in all these muscles it is the superior muscle which forms a pyramidal sheath, the centre of which is occupied by the inferior tendon. It results from this disposition that the tendons of the external and internal tuberosities of the humerus represent a collection of cornets; all holding by a common pedicle, and by their summits to the tuberosities mentioned, and that the surfaces of insertion which these tendons offer to the fleshy fibres are immense, and are in harmony with those of the opposite side.

5th. In general all the muscles which have their tendons in the centre of fleshy fibres, present a structure analogous to that above indicated, and this disposition is connected with a very general law of the structure of these organs, which is that their fleshy fibres are almost entirely placed and attached between the posterior surface of a bone or a tendon for instance, and the anterior surface of another tendon.

6th. I count more than thirty-six sheaths on the upper and lower limbs, but there are only forty-six muscles on the first, if I count right, while there are more than fifty on the second. The specimens I am about to deposit in the Museum of the Medical Faculty of Paris, demonstrate the greater part of these sheaths in an evident manner, and I venture to believe that skilful anatomists will no longer pronounce their existence imaginary and chimerical.—*Feb.* 1825. *Feruss.*

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*Vertebral Fluid in Man and other animals.*—Mr. MAGENDIE read to the Institute, in December last, a memoir on this sub-

ject, in which he states, that in all the living animals used in his experiments, he constantly found the spinal marrow surrounded by a limpid fluid, which flowed out in a jet when a small puncture was made through the dura mater; having also found very frequently a certain quantity of liquid in the vertebral canal of individuals opened twenty-four hours after death, he concluded that this fluid existed in a state of health. Subsequently he recognized the existence of the same fluid in the brain and cerebellum between the dura mater and these organs. This industrious physiologist assured himself by different experiments, that the fluids of the brain, cerebellum, and vertebral column, which otherwise are very similar, communicated freely with each other; this fact was established beyond doubt by the injection of a certain quantity of ink, which, introduced under the arachnoid tunic of the vertebral cavity, penetrated the other cavities with the greatest facility. Mr. Magendie inquires how the existence of this fluid could escape the observation of anatomists. He attributes it to the circumstance of the quantity being very small within the dura mater of the cranium, and to the few researches made on the dura mater of the vertebral column; moreover within twenty-four or thirty-six hours after death the fluid disappears. While Magendie was in attendance at the Hôtel Dieu he several times satisfied himself of the existence of this vertebral fluid. Several physicians having requested him to demonstrate it, he opened the body of a phthisical subject a few hours after death. Not only did he find in the vertebral canal the fluid whose existence he had announced, but the quantity was so great as to distend the membranes containing it, and it spun out with force at the moment the dura mater was pierced; four ounces of fluid were collected from the vertebral cavity alone. In certain regions the distance between the spinal marrow and the membrane which covers it anteriorly, and which has hitherto been thought in contact with it, is half an inch distant, and this interval is entirely filled by the fluid. At the posterior part the interval is never so great. In the inferior part of the vertebral canal the nerves of motion are separated from those of feeling for nearly an inch, and this interval is also filled by the fluid. *The spinal marrow is then throughout its extent surrounded by the vertebral fluid.* It is remarkable that this liquid is more abundant in man, in proportion to his size, than in all the other

mammiferous animals. When on opening a body we do not find any of this fluid, or it is in small quantity, it is because imbibition has taken place, and then the spinal marrow is always softened: the sooner the examination is made after death the greater is the quantity of fluid found.—*Bulletin General, Fevrier, 1825.*

[We have several times remarked the existence of a fluid in the vertebral cavity of human bodies, and sometimes in large quantity, but supposing it to be an effusion consequent on inflammation, paid no particular attention to it. We could hardly acknowledge the existence of this fluid in the healthy state if it rested solely on the dissection of the phthisical patient above mentioned: the subject is certainly one of considerable interest to the anatomist and physiologist. *Ed.*]

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*On the Galvanic Phenomena accompanying Acupuncture.*  
—Mr. PELLETAN, jr. made some first trials which gave no indication of the presence of galvanic fluid; but on repeating his experiments with the galvanometer of Mr. BECQUEREL, he found that the fluid was constantly disengaged from a needle plunged into a painful part of the human body. The quantity of the galvanic fluid is very small, and perhaps it is not the hundredth part of what would be obtained from a single element of the common pile of Volta: still the effects may be rendered sensible, to effect which it is only necessary to make a communication between the needle inserted and the mouth by means of a metallic thread. Mr. Pelletan thinks that it results from all the facts hitherto published, that the curative effects of acupuncture have nothing to do with the galvanism developed during the operation, and he founds his opinion in this respect on the fact that the relief of the patient never appears to be proportionate to the quantity of fluid disengaged, and especially because we obtain very marked effects from acupuncture even with a needle terminated by a non-conducting substance; neither is there any perceptible difference as relates to the relief of the patient when needles terminated by non-conductors are used, or when they are disposed in such a manner as to convey the electricity to the earth. He has seen acupuncture followed by undoubted success, especially in rheumatismal pains. We may add, that Mr. POUILLET has communicated various experiments to the Philo-

mathic Society, from which he concludes that the electricity developed is the result of the oxydation of the steel; no sensible effect is produced when a platina or gold needle is introduced.—*Ferussac Bul. Gen.*

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*Of the Healthy Appearance of the Mucous Surface of the Digestive Organs.* By M. BILLARD, M. D. &c.—The conclusion which M. Billard deduces from his observations on this subject are the following :

1st. The internal surface of the intestines offers certain differences of appearance, according to the age of the individual, and according as it may be examined during or after digestion; but these differences are only modifications of the colour, and of the natural texture of the membrane.

2d. The mucous surface is of a beautiful rose-colour in the foetus, of a milky whiteness in early age, and of an ash colour, or greyish-white in the adult and advanced age.

3d. In individuals who have died during digestion, the mucous coat of the stomach and duodenum, and the commencement of the jejunum, is almost always slightly rose-coloured.

4th. It may be said, as a general result, that the mucous coat of the digestive canal is, when healthy, of a whitish colour, or white approaching to an ash-colour.

5th. The mucous membrane of the stomach never presents a morbid aspect in health, nor black or dark-coloured spots.

6. The muciparous glands are either not apparent, or are so in very small numbers, at the internal surface of the stomach and intestines.

7. These glands may be naturally developed in considerable number, without the health being injured.—*De la Membrane Muqueuse Gastro-Intestinale, dans l'état sain et dans l'état inflammatoire*, &c. 8vo. p. 564, Paris, 1825.—*Lond. Med. Repos.*

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## PATHOLOGY AND THERAPEUTICS.

*Dr. Brandreth's Case of Hydrophobia.*—John Threlfall, aged eighteen, of a strong healthy constitution, was bit in November on the finger by a dog known to be rabid, which after bit a man who died of hydrophobia the succeeding spring. He was seized on Wednesday, the 13th October, nearly eleven months after he was bit, with the dreadful symptoms.

On the morning of Friday, says Dr. Brandreth, he was suffering under violent and incessant spasms of the diaphragm and stomach; continued but ineffectual attempts to vomit: occasionally spitting out, with extreme agitation, a little frothy saliva. He endeavoured to strike every one that came near him; but requested his father and brother to hold his hand that he might not injure them. In a short time he became quieted, but expressed great anxiety at any person passing or moving near him. His pulse at the wrist was quite imperceptible, but the action of the heart very violent, and so rapid as not to be counted; the lower jaw was fixed upon the breast, and the expression of the countenance was indescribably horrid. It was the opinion of every one present, that a very short time must terminate his sufferings. It was agreed, however, to inject the acetate of morphia, into the veins, as recommended by Dr. Booth; and the detail of the case will show the effects produced by it.

At 2 o'clock, 24 minims were injected with a little warm water into the orifice which had been made when he was bled in the morning. This was accomplished with much difficulty, as he became again very violent, and it was necessary to restrain him by absolute force. In a few minutes he was more composed, and told his father he should live a little longer. His pulse became distinct, 140, and the action of the heart had considerably diminished.

At 20 and 50 minutes past 2, the same injection was repeated, which he allowed to be done without opposition; the pulse was now 120, and tolerably firm. Some brandy and water was injected into his mouth with a syringe, but he threw it out with great force. He continued frequently spitting, but endeavoured not to hit any body with it. He now showed his tongue without difficulty, which was excessively dry, and a little furred, and he spoke and answered some questions that were put to him.

At 15 minutes past 3, two moxas were applied over the stomach; he remained perfectly quiet during the time they were burning, and said he felt relieved.

45 minutes past 3, 30 drops were injected. He now eat some bread and butter without difficulty, and attempted to drink some ale, when the convulsions were renewed as soon as the glass was in his hand; but in a few minutes he took a spoonful with great exertion. The pulse was 140, and distinct; his countenance

seemed much improved; he conversed with apparent ease. Pouring water into a basin produced no uneasiness; and when some was splashed on the floor, he merely raised himself to look at it. He complained that his throat was dry, and he had nothing to spit out.

20 minutes past 4.—Rational and composed; pulse 150, and very weak.

40 minutes past 4.—Two more moxas were applied, which appeared to give relief; the action of the heart was moderate, pulse 120; he talked a good deal in a collected manner, and was not at all affected by persons sitting on the bed, or walking about the room.

50 minutes past 5.—Injected a pint of warm water into the vein; the pulse was 90, distinct, and the action of the heart natural.

50 minutes past 6.—30 minims of the acetate were injected: soon after the pulse was 100; the tongue moist and clean; he showed it without difficulty; but had occasionally slight startings. At this time he appeared so much relieved, that some of the gentlemen began to entertain hopes of a favourable result. He continued in this state for an hour, when the pulse became gradually feeble and slow; and when he endeavoured to put his tongue out, it was twisted round in such a manner as to show the under surface.

8 o'clock.—The pulse was imperceptible, and he was bedewed with clammy perspiration. Another injection of  $\mathfrak{z}$ viii. of warm water was had recourse to. There was a considerable quantity of air distinctly felt in the cellular substance about the chest; he made frequent violent attempts to vomit; the teeth were firmly fixed together; and at half-past eight the countenance became of a leaden hue, and he died in a strong convulsion. The body was examined at 7 o'clock of the following morning, ten hours after death.

*Dissection.*—The vessels of the dura mater were gorged with blood; the longitudinal sinus was full of dark blood, mixed with globules of air; the ventricles contained half an ounce of fluid; the brain was very firm, and studded with numerous red points; the choroid plexus unusually turgid; the cerebellum was remarkably vascular, and part of the cineritious substance in a pulpy

state. The blood was coagulated in the lateral sinuses, but not in the longitudinal. On lowering the head, a considerable quantity of fluid escaped from the spinal column. On opening the integuments of the throat, the cellular membrane was found distended with air, which was ascertained, by inflating the lungs, not to proceed from them. The small intestines were quite blown up with air, and exhibited marks of incipient inflammation on their entire surface. The omentum and colon were quite out of sight, and the latter so knotted and small, as in many places to be incapable of admitting the little finger. The upper and outer surface of the stomach was healthy; but upon lifting it up, a large perforation was discovered, capable of admitting the closed hand, from which a clear-coloured fluid had escaped into the abdomen. The pericardium contained  $\frac{3}{4}$ ss. of fluid, and was highly vascular. The right auricle and ventricle were distended with coagula, and the left entirely empty. The cellular and adipose membrane surrounding the aorta contained air. The lungs, liver, diaphragm, kidneys, and bladder, were perfectly healthy. The whole pharynx and trachea were highly inflamed, and the back of the throat was covered with bloody and frothy mucus. The lower part of the œsophagus was deprived of its mucous membrane. The stomach, in general, exhibited considerable marks of disease, and there were many extensive spots of extravasated blood between the coats.—*Edinburgh Med. and Surg. Jour.*

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*Dr. Renwick's Case of Hydrophobia.*—John Duckworth, aged fifteen, was bitten by a dog about a month before he was seized. He was blooded to some extent, had first mercury with opium, and then solution of superacetate of lead, twenty drops on sugar every hour, according to the plan of Dr. Fayerman of Norwich. This, however, produced no visible amendment, and he died.

*Dissection.*—Twenty-one hours after death, by Mr. Halton, in the presence of Drs. Renwick and Jeffreys, Mr. M'Donald, Mr. Jeffrey, Mr. Wilkinson, Mr. Reynolds, and others.

*Examination of the head.*—Upon dissecting back the scalp, it did not exhibit any undue degree of vascularity. The inner surface of the calvaria was turgid with blood. The dura mater presented a general scarlet blush, and upon raising it from the

surface of the tunica arachnoidea, a preternatural adhesion was observed parallel to the course of the longitudinal sinus, upon the left hemisphere of the cerebrum.

The tunica arachnoidea was observed to be opaque in patches, but this appearance was not general. There was not any effusion beneath it, between it and the pia mater.

The pia mater manifested a great degree of vascularity; a general vivid redness pervaded its whole surface. The vessels upon the surface of the cerebrum were gorged with blood.

Several horizontal sections of the substance of the cerebrum, displaying its medullary structure, exposed numerous bloody points, from which blood afterwards oozed; in fact, the substance of the cerebrum appeared injected with blood. Nothing remarkable was observed in the lateral ventricles, nor was there any deviation from the usual appearance in the plexus choroides of each side, or the velum interpositum connecting them. In the commissura mollis, no deviation from its usual consistency was noticed. The pia mater investing the cerebellum was very vascular, but the substance of the cerebellum did not partake of the vascularity; its consistence was softer than usual. The medulla oblongata was natural, the theca vertebralis was very vascular. The bones of the skull were remarkably thin.

*Examination of the Neck, &c.*—The par vagum, carotid artery, and descendens noni were exposed, and no deviation from the natural appearance was observed. The cervical nerves were next examined, but the vascularity so strikingly evident in a former case, (as published by Dr. Brandreth in the last Edinburgh Medical and Surgical Journal, at the dissection of which I assisted,) could not in the least degree be traced, nor did the deep or superficial nerves in the vicinity of the cicatrix manifest any blush. No appearance of inflamed absorbents could be traced. As moxas had been applied to the cicatrices, and had taken deep effect, nothing could be distinguished beneath them.

*Examination of the Pharynx, &c.*—The mouth, fauces, and pharynx were free from inflammation. The œsophagus did not present any unusual appearance until it approached the cardia, where there were considerable marks of inflammation, and some spots of extravasation. The inflammation extended into the stomach for some way surrounding the cardiac orifice, and the extravasation was arranged in a curious speckled manner. Upon

laying open the stomach, the inflammation and extravasation were not general over its mucous surface, but confined to the neighbourhood of the cardia. No aliment was found within it. The liver and spleen were healthy. The kidneys were remarked to be very vascular. The intestines were healthy with the exception of the ileum, which was found inflamed upon its inner surface, about six or eight inches from its termination in the cœcum, and within it there was a secretion resembling bird-lime in tenacity and colour. The exterior of the intestines in the vicinity exhibited a slight blush of inflammation.

*Examination of the Larynx, &c.*—The larynx was natural in appearance. The trachea presented a slight blush of inflammation upon its inner surface at the lower part. The right lung was healthy. A number of firm adhesions were found in the right cavity of the chest. Upon the left side no adhesions were observed, but a considerable effusion of serum was noticed in the bag of the pleura. The pleura costalis of this side was inflamed; the pleura pulmonalis upon the upper and back part of the left lung was also inflamed, and the substance of the lung congested with blood. The heart and pericardium were healthy; but more than the usual quantity of serum was found in the bag of the pericardium.

As in the case of Mr. N. the inflamed appearance of the nerves led to the examination of the spinal column; not observing any thing peculiar in the nerves of the neck in this dissection, it was not considered important to expose either the spinal marrow or the nerves within the pelvis.—*Lancet*.

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*Various Morbid Alterations in new-born Infants.*—N. BRICHETEAU lately read, to the Academie Royale de Medicine, a report on the work of Dr. Vernon, containing three cases, in which the fœtus had been affected with inflammations, similar to those which affect the adult. In one of these, a child, which only lived twelve or fifteen hours, the examination led to the discovery of a pleurisy,—viz. effusion of purulent fluid in the thorax, formation of false membrane on the pleura, redness and injection of vessels. In the second, the infant presented traces of peritonitis; and in the third there had been inflammation of the thymus gland, with the formation of pus in the interior of that organ. M. Bricheteau added other facts to those

of M. Vernon: for example, he quoted instances in which children were born with small-pox; a circumstance confirmed by the experience of various other members of the Academy. M. Desormeaux gave the history of a child who was born with every symptom of an intense enteritis of long standing, and which was cured. M. Hussan lately opened, at the Hôtel Dieu, the bodies of two infants,—one born dead at the seventh month, the other which lived only eight days,—both of which had tubercles, already softened, and in a state of suppuration: the former in the lungs, although the mother was in good health; the latter in the liver. MM. Dupuy and Andral mentioned having found tubercles in the fœtuses of some of the lower animals; and M. Andral, in opening the body of a woman who died of phthisis, found the supra-renal capsules of the fœtus inflamed and suppurated.—*Lond. Med. and Phys. Jour.*

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*Treatment of Cynanche Trachealis.*—The general symptoms of *croup*, the extreme danger of the disease, the frequent inefficacy even of the most active and best-directed treatment, and the striking appearances on dissection, are familiar to every practitioner. There is one fact, however, in the history of this disease, which has not as yet been noticed by authors on the subject; and one means of treatment, which has been repeatedly successful in my own hands, and in the hands of those from whom I first received my information of its utility, which, I have reason to believe, has not come into general use.

The fact to which I refer is, that the exudation of *fibrin* very frequently commences on the surface of the *tonsils*, thence spreads along the arches of the palate, coats the posterior surface of the *velum palati*, sometimes surrounds and encloses the *uvula*; and at last descending, covers the internal surface of the *pharynx* and *œsophagus*, the *larynx* and *trachea*. That this is the frequent progress of the fibrinous exudation I am convinced, from the careful and repeated observation of the phenomena during life and upon dissection.

What is of much more importance, however, than the observation of a pathological fact, is the ascertained efficacy of a means of cure for this disease. Not merely have I repeatedly found the application of a solution of *nitrate of silver* completely successful in removing the fibrinous crust covering the *tonsils*,

velum, and uvula, but I have been led to attribute the rapid alleviation and ultimate removal of all other symptoms to this remedy; even in cases in which, from the severity and peculiar signs of the complaint, I had no doubt that fibrin had already exuded from the lining membrane of the larynx and trachea.

The solution which I employ is *a scruple* of nitrate of silver in an ounce of distilled water. By means of a large camel-hair pencil, this solution is to be freely applied once or twice a day, according to the severity of the symptoms, to the whole lining membranes of the fauces. The surface of the tonsils, or wherever else the fibrinous crust is actually in view, will, of course, be particularly attended to; but I do not hesitate to push the pencil to the lower part of the pharynx.

This remedy, so far from being productive of any irritation, beyond the mere mechanical and temporary one attending its employment, uniformly alleviates the symptoms of croup; such as the difficult respiration, the barking cough, and the peculiar anxiety of the little patient. It has evidently such an effect upon the diseased surfaces, both those which it actually touches, and those which are continuous, as to induce them to throw off the false membrane by which they are covered, and, it appears, also to prevent the further progress of the exudation.—*W. Mackenzie, And. Prof. Anat. and Surg. at Glasgow, in the Edin. Med. Jour. Ap. 1825.*

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*Use of the Kali Hydrudinici in Cutaneous Cancer.*—Dr. GRAFE, of Berlin, has published a few short remarks upon the curative power of the kali hydrudinici in cutaneous cancer. In the case of a woman whose breast was affected with this disease, and who had in vain employed other remedies, an ointment, composed of one drachm of the above preparation and two ounces of simple ointment, was applied once a-day. The wound soon assumed a healthier appearance; the callous edges disappeared, and cicatrization soon began. The strength of the ointment was now doubled. The healing of the ulceration proceeded rapidly, and in the course of a few weeks it was quite well.

In cancerous affections of the upper lip, also, the same preparation has been employed with much advantage.—*Journal für Chirurgie, &c. GRAFE and WALTHER; band 7, stuck i.*

*The use of the Rhus Toxicodendron in Palsy of the Limbs.*—A young man, twenty years of age, of a strong frame, but stupid mind, was admitted into an hospital for a rheumatic affection, which passed off. He subsequently complained of no pain; he ate and slept well. The muscular power of his hands and feet was diminished, and they were almost useless; the fæces were sometimes passed involuntarily. As this symptom was attributed to his natural stupidity, he was threatened with punishment. The power of his limbs was now so completely lost, that he was obliged to be dressed and undressed like a child. In a short time he could not even move his fingers, and was incapable of feeding himself. Various remedies were ineffectually employed; amongst others, the phosphorus in an emulsion. Two days after the use of this article, he became perfectly jaundiced, and was attacked by fever. The relater of the case was now “at his wit’s end;” he knew not what to do, and therefore determined to do nothing. In a few days, the last-mentioned symptoms passed off. The appetite returned, and the palsy of the extremities alone remained. He improved so much in general appearance, that another effort was determined upon, to endeavour to afford him relief. Upon the principle of Celsus, that it is better “anceps remedium experiri quam nullum,” the rhus toxicodendron was given,—a drop night and morning. In eight days the patient could move his fingers; in a month, the arms and feet. The dose of the remedy was gradually increased to ten drops. In two months, he had the perfect use of his limbs. He was shortly quite restored to bodily health; but still remained in a state of mental imbecility.—*Allgemeine Med. Annalen*, Jan. 1825.

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*Case of Poisoning by Laudanum.* By Dr. OLLIVIER, of Angers, and Dr. MARYE.—M. B. P. aged twenty-eight, of a robust constitution, and sanguine temperament, in despair at having lost considerable sums at play, determined to poison himself, and swallowed an ounce and a half of laudanum at once. He at first felt slight nausea, which did not last long; and in the course of an hour became lethargic. Drs. Ollivier and Marye saw the patient *five hours* after the accident, and it would seem that no means had yet been used for recovery. They found him profoundly lethargic, so as scarcely to be roused by being spoken

to very loudly. The face was discoloured, and there were some yellow spots on the lips, tongue, and skin of the fore-arm, attributed to the laudanum. *The pupils were excessively contracted*; the intellectual faculties unimpaired; the answers to questions were made slowly, but distinctly; pulse 109, hard and full; respiration tranquil, with intervals of prolonged blowing. No pain of epigastrium or abdomen; no nausea or vomiting; no dejections, and no urine passed. A slight tremor of the whole body was observed from time to time; no convulsions; no impairment of sensibility. Three grains of tartar emetic were administered in half a glass of warm water, and a purgative enema was injected.

After some hours, the symptoms remaining the same, and the narcotic effects of the laudanum being increased, twelve ounces of blood were taken from the arm. A considerable quantity of strong coffee was given. The pupils continued *excessively contracted*: no evacuations: the coffee was continued; sinapisms were applied, and purgative enemata repeated. There was no particular change in the symptoms at eight in the evening, which was twelve hours after the poisoning, except that the respirations were only four or five in a minute, the pulse less frequent, and *the contraction of the pupils so great that the patient could not see those around him*. The infusion of coffee was continued, and a strong ætherial spasmodic draught given. In the course of the night there was delirium, high fever, and profound somnolency.

The next morning, twenty-four hours after the poisoning, there was a decrease of the lethargic symptoms; the respiration was more frequent; the pulse 116, full and hard. The patient was bled to fourteen ounces, and vinegar and water and lemonade were given as drink. The symptoms remained nearly stationary all the day, but a small quantity of urine was passed. *The pupils continued contracted*. In the evening there was a gradual diminution of the lethargy; several purgative enemata were administered: the night was passed calmly; the sleep was natural; and the next morning all the effects of the poisoning had disappeared.

The successful termination of this case does not seem to warrant any unfavourable remarks on the treatment. The narrators dwell with complacency on the consequences of what

was done, and particularly on the advantage of the venesections. One of the symptoms which is particularly noticed throughout the case is of much interest, when it is recollected that at the trial of Dr. Castaing, for poisoning an individual with the acetate of morphine, M. Chaussier gave it as his opinion, that in cases of poisoning from narcotic substances, the pupil was *necessarily dilated*; and M. Orfila, on the contrary, affirmed that the pupil might be contracted, or dilated, or in its natural state.

The physicians by whom the above case is related took the opportunity afforded by it, of making researches respecting the presence of morphine in the blood and in the urine, and were led to believe in its presence in both. Neither of these gentlemen appear to have heard a word of the cold affusion in such cases, or of the stomach-pump.—*Journ. de Chimie. Méd. Mai, 1825.*

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*Of the Morbid Appearances observed in the Stomach in Phthisis Pulmonalis.*—R. ANDRAL, whose researches in pathology are well known, has lately employed himself in investigating the morbid anatomy of the stomach in phthisis pulmonalis. This last disease is so various, and the title so vague—for many and very different diseases of the lungs are comprehended in it—that it would have been well worth while for so eminent a pathologist, to have explained exactly what he understood by phthisis pulmonalis. We believe, however, that the affections of the stomach which he has described are more or less common to all chronic diseases; and we are much deceived if the facts here announced as new are really so to English pathologists. M. Andral states, that his observations lead him to the following conclusions:—

First, That in a number of cases, a vivid injection of the mucous membrane exists, generally, only at the cardiac end of the stomach, (*le grand cul de sac,*) without any remarkable modification in the consistence or thickness of the subjacent membranes. This injection, which was uniformly situated in the capillary system of the mucous coat, (the veins of a greater calibre, lying in the subjacent cellular tissue, being undistended with blood,) could not be confounded with an injection purely mechanical, the result of difficulty of respiration; it was an injection purely mechanical.

2dly,—In other cases, the mucous membrane was not redder than usual, but had a brownish tinge, and was then in general thickened and indurated.

3dly,—Much more frequently this membrane was softened in different degrees, at one time being also reddened, at another reduced into a pulp, and nearly white.

4thly,—Ulcerations of the stomach were of rare occurrence in the mucous membrane of the stomach.

5thly,—Alterations in the subjacent membranes were rare. Occasionally, however, the cellular tissue appeared indurated, and in two cases the mucous membrane was raised in many hundred points, by tubercles similar to those which are so frequently met with in the small, and less frequently in the great intestine. In both cases there were round the tubercles unquestionable marks of inflammation; swelling and redness in one case, ulceration in the other.—*Revue Medicale, Avril, 1825.*

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*Of the Preservative Treatment of Hydrophobia.*—Dr. WENDT, whose name is well known as a zealous contributor to the advancement of medical science, has lately published a tract in support of the preservative treatment which has been adopted in the Hospital of Breslau against this dreadful malady. The prophylactic measures pursued by Dr. Wendt are the same as those which were first adopted at the Hospital of Breslau, by Dr. KRUTTZE, in 1797. These consist of the application of cantharides on the wound, of the internal exhibition of calomel, and of the external inunction of mercurial ointment, until salivation shall have produced ulceration of the gums. In support of the efficacy of this treatment M. Wendt appeals to an experience of twenty-eight years, and to the guarantee of its success, which may be furnished by the most eminent physicians in that country.—*Darstellung einer Zweckmässigen und Durch, &c.* 8vo. pp. 87. Breslau, 1824.

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*Note on the Employment of Mercurial Æther in the Treatment of Syphilis.*—Dr. CHERON, of Bilboa, and, after him, M. LAGNEAU, whose experience and writings on syphilitic complaints entitle his opinions to the utmost deference, recommend the following preparation as well deserving the attention of practitioners:—

(1.) Take of the superoxygenated muriate of mercury, sixteen grains; of sulphuric ether, one ounce. (2.) Take of white sugar one pound and a half; of distilled water, one pound. Dissolve at a gentle heat, and, after the solution is perfectly cold, add of mercurial ether, (No. 1.) one ounce. The dose is from two to four drachms, morning and evening. This dose may be progressively augmented.—*Archives Génér. Mai, 1825.*

*New Method of Treatment in Diseases produced by Lead.*  
By M. RANQUE, Physician to the Hôtel Dieu of Orleans.—It seems scarcely credible, that the treatment of *colica pictonum* at the Hospital of la Charité in Paris, should at this day be conducted according to the precise rules of the founders of that institution, rules which leave nothing to be performed by the mind of the practitioner, but bind him down to apply particular means at particular hours, without regard to the particular case of the person to be treated. Notwithstanding the ambiguity of this method of treating a complaint *by the clock*, it is not pursued at the neighbouring hospitals. At the Hospital of *Beaujon*, local bleedings and laxatives are trusted to; and at the *St. Anthony* the cure is sought chiefly, if not entirely, from the sulphates of antimony. M. Ranque informs us, that in Prussia the disease is combated by the administration of large doses of oily and fat substances; and that the practice in Austria, like that of the English, consists chiefly in giving opium freely, either alone or combined with purgatives. M. Ranque considers all the affections produced by lead, if unaccompanied with pyrexia, thirst, or dryness of tongue, as neuralgic; and when those symptoms are present, he considers them as consequent on the neuralgic affection. He traces *all* the varieties to a primary affection of the ganglionic nerves of the *abdominal* viscera, (*des plexus de la portion abdominale du trisplanchnique,*) and the simple practice which he follows, is founded on the power of modifying pathological states of these nerves, by acting on the portion of the cerebro-spinal nerves, which is spread over the cutaneous and gastro-intestinal surfaces. With this intention, which he thinks has been fulfilled in more than three hundred cases, he applies epithems to the abdominal and lumbar regions, and an anti-neuralgic liniment and lavement, giving the patient demul-

cent and mild drinks. The composition of the abdominal epithem is as follows:—

Diachylum gommé, (composed, we believe, of mucilage, litharge, decoction of the *gladiolus communis*, pitch, turpentine, wax, &c. &c.) ℥ss. theriaca ℥ss. emplastrum conii ℥ij. camphora ℥j. sulfur (pulv.) ℥ss. Mix these well together, with the assistance of a moderate fire; spread the amalgam on a skin the size of the abdomen, and powder the surface with camphora, antimonium tartarizatum āā ℥iss. sulfur (flor.) ℥ss. previously mixed together.

This epithem is renewed in two days, unless the pain is previously relieved; but is taken off as soon as any pustules appear.

Another epithem is applied on the lumbar region, extending from the last dorsal vertebra but one down to the sacrum: it is composed of one part of the conium plaster, and two parts of the diachylum gommé, with the addition of camphor and sulphur; and is directed to be worn five or six days. The liniment prescribed by M. Ranque consists of aqua lauri cerasi ℥ij. æther sulphuricus ℥j. extractum belladonnæ ℥ij. These applications, it is probable, are serviceable, inasmuch as they may possess both sedative and counter-irritant properties. The enema employed by M. R. with the intention of overcoming the spasm of the lower intestines, and so removing the obstinate constipation, consists chiefly of oil of almonds or of olive oil; to which is added an ætherial tincture of the leaves of belladonna. The mucilaginous drinks are intended to sheathe the mucous membrane, with the object of preventing inflammatory action and paralysis. When the pain is seated in the head, M. Ranque sometimes directs the abdominal epithem to be applied behind the neck, and causes the temples to be rubbed with the liniment. If the chest is chiefly affected, the epithem is applied to the back, and the liniment is rubbed into the axillæ.

There does not appear to be much novelty in this ‘new method’ of treating diseases produced by lead, except in the form of the applications; but, as the adoption of the form may occasionally be serviceable in these troublesome affections, we have thought it worth while to give some account of it.—*Arch. Gen. de Med.*

*Case of Poisoning by Opium, successfully treated by Dr. ALISON of Edinburgh.*—On the 5th of February, 1825, Dr. Alison was called to see a gentleman's servant, a strong man, aged thirty-five, who had swallowed, as he was said to have confessed about twenty minutes before, *one ounce and a half of laudanum* in a fit of despondency succeeding intoxication. He was found speaking incoherently, and as if intoxicated, but without any appearance of *stupor*; the pulse full, strong, and frequent; the skin warm, and the face somewhat flushed.

During the space of an hour, various attempts were made to empty the stomach by means of mustard, sulphate of zinc, tartar emetic, &c. but in a great measure ineffectually; for although vomiting did take place several times, yet the quantity of matter evacuated hardly exceeded a mouthful at a time. At length all expedients to excite the action of the stomach ceased to have any effect, and the man fell into a state of profound coma. About an English pint of tepid water was now introduced into the stomach, by means of an instrument recommended by Mr. Bryce; copious vomiting immediately succeeded, and a repetition of the process was followed by the same effect.

After the evacuation of the stomach, the breathing became more regular and less stertorous, but the livid flushing of the face continued, and the only signs of sensibility manifested, were frequent and feeble efforts to scratch the legs and thighs. The pulse was still full and frequent, and the skin warm on the upper parts of the body. The patient was now stripped naked, and cold water was dashed over his head and shoulders. By this he was powerfully excited; he sprang from his seat and fell forwards, gasping strongly and repeatedly; when raised, he opened his eyes and looked wildly around, and being rubbed dry, walked with assistance into an adjoining room, where he lay down and had hot bottles applied to his feet, which were now very cold.

For nearly an hour after this he was very restless, tossing about in bed, and scratching himself incessantly, but could hardly be induced to speak. At length he fell into an apparently tranquil sleep; the flushing of the face entirely subsided; the skin became rather cold; the pulse smaller, but still firm; the breathing slow, but regular and easy.

In this state he continued for more than an hour, easily rous-

ed and sensible when spoken to. The medical gentlemen therefore thought they might with safety commit him to the care of the attendants, and withdrew. In about four hours after, however, Dr. Alison was recalled, and found him with all the appearance of a dying man: he was quite insensible; the countenance pale and ghastly; the lips livid; the jaws fallen; the skin generally cold, although warmth had been assiduously applied; the respirations four or five only in the minute, and the inspirations performed with a convulsive start; the pulse much smaller than before, but still tolerably firm.

From this state he was gradually roused and restored by dashing cold water on the face, by applying ammonia, (the *liquor amm.*) to his nostrils, by frictions with the same on the chest, by the application of hot bottles to the feet and stomach, and by the exhibition of hot coffee, &c. &c.; so that in about twelve hours from the time the laudanum had been taken, every alarming symptom was at an end.—*Edin. Med. Journ. April, 1825.*

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*Neuralgia Cured by the External Application of Belladonna.*

—CASE I.—John Gale, hospital-serjeant, 66th regiment of foot, thirty-nine years of age, delicate, subject to inflammatory attacks, went to bed on the night of the 2d of March, 1824, quite well. He awoke with a strange pulsation and tingling over the *right* eyebrow, which soon increased to a sensation of heat and soreness, and at length to a most extraordinary pain, extending across the temple, and shooting down the cheek. This was attended with an involuntary constriction of the eyeball, and a copious effusion of tears. The focus of the pain appeared to be the supra-orbital foramen of the os frontis. The paroxysm lasted two or three minutes, when there was an interval of partial, and sometimes of complete ease, for five, six, or ten minutes; then the same symptoms recommenced, and after four or five hours of alternate torture and comparative happiness, the disease ceased; the forehead and temple, however, remained numb, and tender to the touch, the day after.

From a belief that the disease was, in some way, connected with vascular fulness of the system and topical congestion, the depletory plan of treatment was carried into effect. Blood, in considerable quantities, was abstracted, generally and locally; the bowels were briskly purged; blisters were applied to the

*right* temple, and behind the ear; the patient was confined to bed, was kept on spoon-diet, and all sources of nervous irritation were carefully avoided.

This mode of treatment was persevered in for about *ten* days, but with very little advantage; for, at the end of this time, the paroxysms were neither reduced in frequency, nor moderated in violence.

On the eleventh day, about ten grains of the extract of belladonna, moistened with a little water, were rubbed over the *right* eyebrow, for about three minutes, during a violent paroxysm. The result was, an instantaneous abatement of the pain: it returned, however, in half an hour; but on that night, (the *twelfth* from the first attack,) the patient slept better, and had fewer and shorter fits of pain than on any preceding one since the commencement of his illness.

The same application was repeated on the next night, and with similar good effect; on the fourteenth night there was an intermission; on the fifteenth the medicine was again used, and the patient passed the night tolerably well. The disease continued to yield; and finally, in *three* weeks, (that is, we presume, at the end of *one* week from the first application of the belladonna,) the man was quite well.

Since that period, (now thirteen months,) the disease has returned two or three times; the attacks, however, were mild, and always yielded to the same application.

CASE II.—A lady, fifty years of age, plethoric, but healthy, was attacked, (on the evening of the 26th of March last,) with a tingling over the *right* eyebrow, which gradually changed into a convulsive movement of the levator palpebræ and orbicular muscles, with lancinating pain over the brow, radiating from it across the temple, down the cheek, up the forehead, and into the orbit of the same side. The acuteness of the pain continued only three or four minutes, was followed by about a quarter of an hour's ease, and then came on as before. The succession of paroxysms, constituting, as it may be termed, *the fit* of the disease, lasted about six hours; and in this, as in the preceding case, an unpleasant feeling of numbness, and slight irregular twitches of the muscles of the eyelids were left behind.

For ten days, (during seven of which sleep was entirely banished,) the disease was suffered to proceed in this manner, no

medicine or medicinal application being employed during this time, but some fomentations to the part. On the eleventh day, about the size of a pea of the extract of belladonna, moistened with a little water, was rubbed with the finger over the seat of the most excruciating pain, at the supra-orbital foramen. The friction was continued about five minutes, when the iris of the *right* eye became considerably expanded; it was then discontinued. In less than ten minutes the lady exclaimed, *I feel no pain*: in about five minutes more, the convulsive twitchings of the muscles of the eyelids also ceased; and, from that hour to the present time, (April 17,) the patient has not had the slightest return of the disease, and is now in excellent health.—MR. HENRY, *Assistant-Surg. 66th regt. in Lond. Med. Journ, June, 1825.*

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### SURGERY.

*Ophthalmia cured by Acupuncture.*—M. CLOQUET lately presented to the Royal Academy of Medicine the case of a young woman, who had been affected for a very long period, with chronic ophthalmia of the left eye, with puriform discharge from the eyelids. The eye was constantly closed, and the affection complicated with acute pain in the orbit and in the head. A variety of means had been employed without success, when M. Cloquet had recourse to acupuncture. He inserted two needles in the temporal region of the same side; they caused but little pain, and at the end of a few days the inflammation had sensibly diminished; but it was especially the pain in the orbit and head which disappeared almost instantly. In a few days they again returned. He then introduced another needle into the middle of the forehead, and left it there. The eye speedily improved, remaining open, the pain disappeared. There existed at the same time an eruption on this side of the face which was almost entirely removed.

M. Husson communicated to the same body the case of a very stout man, who was admitted at the Hôtel Dieu for an ophthalmia of the right eye, which was entirely closed and extremely painful. This inflammation, which had resisted during eight days all the most active remedies, disappeared very rapidly on the introduction of two needles into the thick part of the eyebrow, on the right side.

M. Nacquart afterwards communicated the case of a young lady affected with a painful ophthalmia of long standing, in which the acupuncturation had been practised twelve days before, without any benefit resulting. It appears, however, that from the modification in the operation employed, it rather resembled the introduction of a seton, than the acupuncturation properly so called.—*Anderson's Quarterly Journal*, July, 1825.

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DR. MARTLAND'S *Case of Staphyloma cured by Tapping*.—J. Snape, aged 38, was seized with severe pain in the head while in his Majesty's service at Gibraltar, in 1812, terminating in quotidian intermittent, of two months duration. In April, 1820, Dr. Martland was consulted, and found a cataract of both eyes. In October he cut up the lens of the right eye, and pushed a small portion through the pupil. The operation was repeated twice, and with complete success. On the 14th of December an oculist extracted the cataract from the left eye, with some difficulty, owing to a partial adhesion of the capsule to the iris, which was unfortunately lacerated, with the escape of some of the vitreous humour. Prolapsus of the iris, with violent inflammation, took place the first day after the operation, and continued upwards of six weeks, leaving the patient quite blind of this eye, though very active measures had been pursued to reduce the inflammation, and the unreduced portion of the iris was destroyed by lunar caustic. On the 30th April, he complained of pain darting through the left temple and forehead, with sense of throbbing, as well as of pain, at the bottom of the orbit of the left eye. The conjunctiva was very vascular, and a large tumour was situated on the ball of the eye, opposite the external canthus, when the eye was directed to the opposite side. The tumour resembled that of the sclerotic coat delineated in Travers's Synopsis, pl. 1. fig. 7, but was rather broader at the base, and of a deeper colour. The iris lay very near the cornea, and had become adherent to it where the incision was made. After some preparatory measures, the eye was tapped by means of a small cataract needle, and some of the vitreous humour escaped. The pain was relieved in half an hour; but recurred in the night as bad as ever. The inflammation of the eye became more extensive, and the tumour increased in size. Bleeding, purging,

calomel, opium, and antimony were exhibited. On another puncture being made in about three weeks after, viscid fluid escaped with much relief. The inflammation now subsided, and the man resumed his trade of weaving. In a few days the tumour again filled, and an additional tumour appeared, and the head-ache returned. On tapping the tumours much viscid humour was again evacuated, with instant relief. In a week, on a recurrence of all the bad symptoms, paracentesis was repeated with the usual good effects. We cannot follow the author through his diurnal detail of symptoms and operations; but on the 16th of August, we observe that Dr. Martland varied the paracentesis. He made a perpendicular incision through the coats of the eye, fully one-third of an inch in length, and about two lines and a half from the temporal edge of the cornea. A great quantity of vitreous humour soon escaped, causing the eye to sink a good deal in the socket, and become very flabby. In spite of this the tumours soon filled again, and it was necessary to tap them six or seven times afterwards. On the 23d of February, 1824, no vestige of the tumours remained, nor has there been any pain or inflammation since; there is a cicatrix where the incision was made.—*Idem*.

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*Uterus, Extirpation of*,—The operation for the extirpation of the uterus has, since the beginning of the present century, been performed in Germany no less than six or seven times. The first of these operations was, (in 1808,) successfully performed by Osiander in Gottingen, on a woman from whom he had, seven years before, removed a cancerous tumour of the uterus.

In 1813, the operation, or rather only the removal of a tumour from the *cervix* uteri, was performed by Professor Rust of Vienna, but unsuccessfully, as the patient only survived eight days after it. In 1817, Langenbeck extirpated a *prolapsed* uterus, and with success. In 1822, Santor performed the still more difficult operation of removing an *unprolapsed* uterus, and since that time the same operation has been attempted in Berlin, Hanover, and Vienna. Of these attempts, two at least were unsuccessful—the result of the third is not yet published.

This operation, though of late years revived, is not one of modern invention. In *Sue*, (*Hist. des Accouch. Par.* 1786,) we find an account of the extirpation of the uterus having been

successfully performed as early as the year 1560, by Andreas à Cruce, physician and professor at Venice. Nay, the removal of the prolapsed uterus was successfully effected twenty years before that period, (in 1540,) by one Carpus; and, in the beginning of the seventeenth century, the same operation was also performed by Zacutus Lusitanus. In the same author also, (*Sue*,) are to be found several other cases, both successful and unsuccessful of this operation, which prove that it is not one of modern date.

These accounts, however, are not very explicit, and contain neither a description of the operations, nor a very accurate statement of the circumstances under which they were performed; and it is to Osiander, undoubtedly, that the merit belongs, of having improved this part of surgical science, and of having first systematically described the manner in which the operation should be effected.—*Edin. Med. Journ. April, 1825, p. 397.*

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*Dr. Hælscher's Case of Extirpatio Uteri.*—Madame Von C—, aged 30, of good constitution and full habit, the mother of four children, (having been married at fifteen and enjoyed good health,) miscarried during her fifth pregnancy, in consequence of an accident, which was followed by inflammation, vaginal discharge, cough, and pains in the chest. Examination per vaginam, gave great pain, and was always followed by hemorrhage. The catamenia appeared every three weeks, and there were occasional hemorrhages. The uterus was found by Dr. Hoelscher to be surrounded by a fungus, and he determined on the operation.

“The bowels and bladder were first emptied, and the patient was then placed in the horizontal position, with the legs separated from each other. An assistant pressed down the uterus and pushed the intestines upwards, whilst the operator conveyed a scalpel along the middle of the fore finger of the left hand to the mouth of the womb. The instrument was then carried round the fungus, and the uterus separated from nearly the whole of its connexion with the vagina. The patient lost at this step of the operation, about five or six ounces of blood. The operator now introduced his left hand into the vagina, and seized the carcinomatous mass, but was unable to pull it down. Although the description of Sauter's operation had determined Dr.

Hoelscher not to occupy himself much longer in attempting to effect a descent of the uterus, yet he judged it advisable to convince himself by one more trial, whether it was possible or not. The operator had provided himself with several pins of brass, of the size of a crow's quill in thickness, about a foot in length, easily bent, and rather sharp at the point. One of these was conveyed, with the point slightly bent, along the fore finger of the left hand, and attempted to be pushed above the carcinomatous mass, into the cervix uteri. The left hand was then conveyed to the other side, in order that the instrument might be carried more easily through the cervix, the point of which the operator hoped he should be able to lay hold of with a wire forceps, (or a piece of wire bent into the form of a forceps,) and thus effect a descent of the womb. But he was soon convinced, that the carrying of the wire through the substance of the uterus, to its outer part, so as to meet the instrument would be attended with great difficulty and loss of time; and even if he succeeded in getting it through, it would most probably slip, and therefore he relinquished the trial. He then determined on immediately removing the carcinomatous mass, which considerably impeded the performance of the operation. A circular knife was then conveyed along the fore and middle finger of the left hand, to that part of the uterus from which the fungus had been torn off, and then carried, in an oblique direction, from right to left. After the extirpation of this mass, a hemorrhage, to the amount of two or three ounces, took place, which was, however, soon stopped by the application of a sponge moistened with vinegar. The uterus was now more easily to be got at, and the operator introduced a sharp-pointed knife along its outer edge, as far back as the fungus. An opening being thus made, the circular knife was introduced, and the uterus separated from all its lateral connexions, after which it was easily removed. The operation lasted thirty-five minutes, and the whole quantity of blood lost amounted to no more than nine or ten ounces. The intestines did not in the least protrude. Some fine sponge was introduced into the vagina, and the patient conveyed to bed. Her hands, feet, and countenance were cold, and the patient was evidently much enfeebled by the operation. Some wine and water, together with an anodyne, were given. The patient passed a very restless night, and on the following morning was attacked with vomiting; the abdomen became distended, and in

the lower part, painful to the touch; the pulse feeble; and in twenty-four hours after the operation she died.

“On opening the abdomen, not the slightest trace of hemorrhage was to be seen. The opening in the peritoneum had closed to the size of a hen’s egg, and would have soon united if the patient had survived. There was no sign of carcinoma in any part, and the ovaries were quite natural. The intestines were distended with flatus.”—*Gräfe und Walther’s Journal der Chirurgie.*

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*Large Calculus passed from the Female Urethra.*—A case is related by Dr. SOMMER, in the *Journal der Chirurgie und Augen-Heilkunde*, (band 7, heft i.) in which a female, of nineteen years of age, after suffering from severe pain in passing her urine, and occasional retention of it, had a catheter introduced, by which a stone was discovered obstructing the passage. The urethra was dilated by forceps; the stone was grasped by them, and withdrawn. It was found to weigh ten drachms and a half; its long diameter measured two inches and a quarter; the transverse diameter was one inch and a quarter: it was of a whitish-grey colour, tolerably hard, and rough upon its superficies. The patient soon recovered.

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### MATERIA MEDICA.

*Euphorbia Lathyris*—*The Expressed Oil, a mild and active Purgative.*—The *Euphorbia lathyris*, or *caper spurge*, is a native of the south of Europe, and was first introduced into our gardens, where it is now not uncommon, about two centuries ago. It is naturalized somewhere in Bedfordshire, and in most old herb gardens—it flowers in May and June.

From the *seeds* of this plant our oil is obtained by expression, which has lately been proposed as a substitute for the Croton Oil, by Dr. Calderini of Milan, who states, that in purgative power it is little inferior to the latter; and that it is preferable in this respect, that its exhibition is not attended by any of those unpleasant symptoms which the acrid and irritating qualities of the Croton Oil are so apt to occasion.

When filtered, this oil is, in appearance, very similar to castor oil—it is inodorous, limpid, and neither acrid nor disagreeable to the taste. By long keeping, however, or by expo-

sure to heat, it becomes turbid, dark-coloured, acrid, and afterwards rancid. It burns with a clear flame without smoke, is insoluble in alcohol, and forms a soap with alkalis.

For an adult, the dose is from four to eight drops, which may be given in an emulsion, or in any other convenient vehicle—its action upon the bowels is certain, strong, and prompt; but at the same time mild, for it neither occasions vomiting, tenesmus, or pain—unless, indeed, it be in a rancid state, when it becomes drastic, and produces griping.

The application of this plant to medical uses is not new; from Mathiolus, who wrote in 1562, we learn that, in his time it was commonly known in the shops by the name of cataputia, and it is particularly noticed by Gerarde in 1636.

Dioscorides and Galen also are supposed to have mentioned it, as have various other writers, all of whom, however, agree in speaking of it as a very violent purgative, contrary to what we would be led to believe from the statement of Dr. Calderini. This apparent discrepancy arises, in all probability, from this circumstance, that the *entire seeds* were formerly used, and not the *oil* simply, as now recommended.—*London Medical Repository*.

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### OBSTETRICS.

*Laceration of the Uterus in Parturition.*—In the years 1819–20–21, 8600 patients were delivered in the Lying-in Hospital of Dublin, and of this number *twenty* suffered *laceration of the uterus*.

Of these twenty cases, *five* occurred in women parturient with *female* children, and *fifteen* in women parturient with *male* children. The *sex* of the infants, therefore, would seem to have a considerable influence in the production of this accident—a circumstance doubtless connected with the *greater size of the head* in the generality of *male* infants.

Thus, of sixty *male* and sixty *female* infants, born at full time, and examined by Dr. Clarke, the averaged circumference of the *head* in the former was found to be *fourteen* inches, and in the latter only *thirteen and five-eighths*; and of one hundred and twenty infants examined by the same physician, the circumference of the head exceeded *fourteen inches and a half* in six only, all of whom were *males*.

From examination after death, it would appear that the *neck* of the uterus is the part most frequently lacerated, and that the rent is generally in a transverse direction.—*M. Kever in Lond. Med. Repos.*

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*Case of Cæsarian Operation*—By Dr. SCHœNBERG, of Naples.  
—Elizabeth Hugh, twenty-eight years of age, of very short stature, and when a child affected with rickets, had for several years past been subject to very severe attacks of convulsions, and on the 7th of August, 1823, towards the end of her first pregnancy, labour pains came on. At the same time she was seized with convulsions, which resisted every means I could try, and continued for thirteen or fourteen hours, wherefore I determined on delivering the woman. On a close examination, I found that the mouth of the womb was dilated, that the waters had escaped, and the head of the child was foremost, but that the pelvis between the rami of the ischium and pubes was extremely narrow, not more than two inches in width. Under these circumstances, I called in a physician-accoucheur, who found the woman in this state, but had not an opportunity of ascertaining if the child was alive or not; a circumstance which was to decide whether we should have recourse to the Cæsarian operation, or the perforation of the head of the child. In consequence of the uncertainty whether the child was alive or not, and the frequency of the labour-pains, we determined to wait till the following morning. August 8th.—The woman rested the whole of the night, and this morning felt considerably refreshed. In our examinations we could distinctly feel the motions of the child, which lay with the face foremost, and thus we decided on the Cæsarian operation, which was performed at two in the afternoon by H. Leuch, one of the surgeons to the hospital. An incision was made in the course of the linea alba, from about an inch below the naval to the pubes, and with a second the uterus was opened when in a state of contraction, a labour-pain having occurred at this step of the operation. The fingers were then introduced, the opening in the uterus enlarged, and the placenta detached. The membranes were now ruptured, and the child in part removed from the womb; but just as the head was about to be withdrawn, the uterus contracted round the neck of the child, and it was with some difficulty that the head could be extricat-

ed. The intestines now fell forwards, and were obliged to be kept back by an assistant; the placenta was then withdrawn, but no arteries were secured, as the bleeding was very slight. The uterus was rendered as clean as possible, all the coagula being removed, and the parts were placed in their natural position. The integuments were kept together by means of sutures, and strips of adhesive plaster; and over these, compresses and a broad bandage were applied.

The child, when removed, was alive, since it moved, and made continued efforts, during half an hour, to breathe: but notwithstanding all our efforts, it shortly died. The woman went on remarkably well; the lochial discharge appeared in the usual quantity; and, in forty-eight days from the operation, she was perfectly well.—*Salzburgh, Medicinisch-Chirurgische Zeitung, May 27, 1824.*

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*Spanish Physicians.*—It is generally believed, that the physicians of Spain are not so respectable a body as those of England or France. That they are not so wealthy as the former, nor so consequential as the latter, may be allowed; but as a profession, protected as they are by laws, to support their rank, and exclude ignorant pretenders, they are not inferior in respectability to their other European brethren. The Times newspaper in 1820, stated that the fee of a Spanish physician was three rials, and the paragraph has been going the round of the papers. What the Times stated was true, but not the whole truth.

The physicians of Spain are upon an establishment somewhat like the English clergy; they possess livings of from one to five hundred a year. Each, on being approved of by the examiners, is, according to his interest and talent, appointed to a certain village or town, for which he receives a yearly salary. He is obliged to visit all the sick of his district, and cannot demand a greater fee than three rials; but this he is allowed. If, however, his fame becomes extended, and he is sent for to any post out of his district, then his fees are unlimited, and in this case thirty guineas have been given. By this wise regulation of the Spanish government, the poor are not driven to quacks and hospitals; but receive every necessary attention from their proper officer of health, for a trifling expense, and the physicians are

all supported in their proper rank. The surgeons are not so respectable.\*

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### OBITUARY.

*M. Bécларd.*—France has to deplore the recent loss of M. Bécларd, the greatest, perhaps, that, since the death of Bichat, has been sustained by Surgery. The journals of that country have tendered their various homage to his public and private virtues, and his honoured remains were attended to the grave by a crowd which excited curiosity even in the procession-loving city of Paris. A few particulars relating to his life, selected from the different periodical publications of that capital, will, we are assured, be read with interest.

Bécларd was the son of a respectable tradesman of *Angers*, and was born in 1785. His parents had several other children, and Pierre-Augustine was at first destined to the same rank in life as themselves. The talents which he very early displayed, and his remarkable attachment to study, led them however to promote his education to the utmost extent of their means. At that time Bichat was in the zenith of his glory, his name and his works were spoken of in every province, and the young Bécларd felt that sort of envy, which both in generous and aspiring minds is more allied to virtue than to vice. His aspirations were, however, for some time suppressed, by the vain attempts of his father and mother to subdue the ambition of the future anatomist into something more suitable to a tradesman: but after one or two unsuccessful trials to make him first a dealer in hardware, and then a merchant, and then a clerk in a wagon office, being assured by his successive masters that they could make nothing of him, the despairing old people allowed him to become a pupil at the *Hôtel Dieu* at *Angers*. In this situation, the passion for study, which had been so fatal to his prospects in pursuits unconnected with science, was soon productive of the greatest effects; and in 1808, after passing four years at the hospital of *Angers*, so as to acquire the esteem of all around him, he removed to Paris.

In the midst of the distinguished surgeons of the large hospitals of that city, his abilities soon became conspicuous; he gained

\* For this article we are indebted to an intelligent practitioner, who has heretofore liberally contributed to the pages of this Journal.—*Eds.*

one anatomical honour after another, and was successively selected by M. Roux for the office of his assistant-lecturer (*répétiteur*,) at *la Charité*; and chosen demonstrator to the *Faculty*, and *Chef des travaux anatomiques*. In 1813, on the occasion of being created Doctor in Surgery, he presented a thesis containing several original opinions in physiology and surgery. His progress was a little interrupted, by his failing to obtain the office of assistant-surgeon to the *Hôtel-Dieu* in 1815; but his merits were considered quite equal to those of M. Marjolin, the successful candidate, and three years afterwards he was elected by the votes of all his preceptors, and according to the wishes of all the pupils, to the chair of Anatomy in the Faculty of Medicine of Paris.

After attaining this important situation, he appears to have devoted his whole time and his whole soul to such a continued series of laborious studies, connected with his office of a teacher, as caused his lectures to be crowded with students, and eventually shortened his existence. Few men appear to have been more devoted to public duty, and more ardent in the pursuit of science, or more negligent of ornament, or more careless of the emolument which might have been derived by attending less to his pupils and more to the public. As a lecturer, he was clear and precise, without being ambitious in his style. His conception was rapid and powerful, his memory extensive, his judgment correct, and his elocution agreeable. His hearers forgot the professor amidst the richness and the beauty of science which he displayed to them; and some idea may be entertained of his industry by the fact, that, notwithstanding his great abilities and his long familiarity with his subject, the preparation for a single lecture, often occupied him four or five hours. He seems to have had a pure passion for science, unmixed with any strong desire for personal distinction, to have welcomed knowledge, from whatever quarter or country it came to him; and to have been uniformly more desirous to *learn* the truth, than to be the *discoverer* of it—an undoubted feature of a great mind.

His principal published works are, a memoir *sur les Acéphales*: a memoir of considerable length *sur les Blessures des Vaisseaux*: a memoir on Osteology, the fruit of much observation and research: an essay on Embryology, published in the name of his brother: numerous experiments on local affections of the nerves,

published in a thesis by M. Descot, in 1822: and a work on General Anatomy, for the use of his pupils. He was the author of many articles in the different *Dictionnaires de Médecine*; and of several papers contained in the *Bulletins de la Société d'Emulation*; and other collections. He also edited the second edition of Bichat's *Anatomie Générale*, adding many observations of his own: assisted by M. Jules Cloquet, he translated Mr. Lawrence's work on Hernia into the French language; and in conjunction with the same author he had commenced a series of anatomical plates.

It is gratifying to find that his private character was no less distinguished by virtues, than his public character by talent and labour. He was not naturally of a communicative disposition in conversation, but when a stranger had got over the coldness of his exterior, he invariably found him obliging, and ready to aid others by his advice, his knowledge, and access to his valuable library. As a public examiner of pupils, he was just, without undue severity; and, in that department of duty, repaired many abuses which had been greatly complained of. When it was thought proper, for political reasons, to *re-organize* the *Faculté de Médecine* in 1823, very general anxiety was felt for the probable removal of M. Béclard from his office; but, notwithstanding some intrigue and manœuvring, the commanding merit of the professor preserved him his chair in the newly-modelled school. As a son, a husband, a father, a brother, and a friend, he seems to have been rich in amiable qualities, and eminently beloved.

In the midst of his labours and his duties, in the height of his fame, and in the prime of his life, he was attacked by a disease which, in eleven days, deprived his family of his protection, and his pupils and his country of his services. His too great assiduity in his pursuits had, for some time, been apparently productive of a kind of chronic inflammation of the stomach; to this succeeded an acute cerebral affection, accompanied with erysipelas, which, notwithstanding all that art and all that friendship could suggest, was fatal. He is said to have described the nature of his complaint in a very collected manner to those around him, and his description was found correct, on examination after death. With every thing to attach him to life, and

every capability of enjoying existence, he saw the sure approach of death, and met it with becoming firmness and resignation.

His funeral was attended by an immense concourse of students, all anxious to express their respect for the memory of a man whose life had been consecrated to their improvement. The Faculty of Medicine, deputations from the Royal Academy, and the different societies of medicine, and a crowd of colleagues and friends, accompanied the procession to the cemetery of Père La Chaise, and in this beautiful place of repose for the dead, various honours were paid to the defunct. First M. Pelletan, in the name of the Faculty of Medicine, then M. Pariset, in the name of the Academy, then M. Roux, in the name of the Section of Surgery, then M. Adelon, in the name of the numerous friends of the deceased; and, lastly, a young pupil, in the name of the rest of the pupils; deplored the end of a life which had been so useful and so short, and enumerated the talents and virtues of the dead.

M. Richerand was to have spoken for the Faculty of Medicine, but was unable to do so; and M. Pelletan being called upon on the spot, delivered a kind of funeral oration extemporaneously, and with much feeling. The following extracts are given in the *Revue Médicale*; and are not without interest to all men who aspire to posthumous honours by honourable means:—

“Béclard! thou hast been our colleague, our associate, our friend. The Faculty of Medicine had selected from among us him whom thou lovedst the most, to express in this place the feelings of us all. But he has been overcome by his grief, and unable to follow us; yet the regrets of the faculty shall not be silent, although his voice has been suppressed by tears. . . .

“How brilliant, but how short, hath been thy career! Distinguished among thy fellow-students whilst yet a pupil, thy reputation was established by numerous proofs. Early elevated to the rank of professor, thou wert a model for others to form themselves by. Justness of thought, excellence as a lecturer, wisdom of opinions, profound erudition, indefatigable zeal for science—all these precious qualities, from which we were daily profiting, were united and ennobled in thee by their union with good faith, with disinterestedness, with humanity. . . .

“I see, in this immense crowd, more than one unfortunate

being whom gratitude has brought hither, that through the close and far-extended ranks of thy pupils, he may at least catch a sight of thy tomb. . . . .

“ But what are my broken sentences—what are these words lost in the air, when all that they would convey is a thousand times more imposingly expressed by the spectacle around us! Thy colleagues and thy friends have been unwilling to part from thee until the last moment. The immense assemblage round thy grave, the faculty, with all its professors, with all its fellows, with all its numerous pupils, so many distinguished men, so many weeping friends, declare the loss we have suffered, and the void which has been made among us. And when I turn from these to thy despairing family, where would they find consolation, if their private sorrows were not softened and mitigated by this great expression of the public grief?

“ Farewell, then, Béclard! thou art worthy of our praises, for thy friends were many: we will preserve thy remembrance: thou hast bequeathed to us thy example.”

The pupils of the deceased professor, who had insisted on bearing his coffin on their own shoulders to the grave, relieving each other by turns, terminated their pious offices by covering the precious remains with earth, and closing the grave with their own hands.—*Lond. Journal*.

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## AMERICAN INTELLIGENCE.

[The following communication was received when the present number of the Journal was nearly concluded. As the information it contains is so interesting to practitioners of medicine, we have preferred inserting it here, rather than delay its publication until the appearance of the next number.—*Eds.*]

*Observations on CORNINE, (an Alkaline Principle, recently obtained from the bark of the Cornus Florida, by GEORGE W. CARPENTER, of Philadelphia.)* By SAMUEL GEORGE MORTON, M. D.

THE bark of the *Cornus Florida*, or Common Dogwood, has long been used as a febrifuge by the country practitioners of the United States, and with very marked advantage. This fact in-

duced Mr. GEORGE W. CARPENTER, an ingenious and enterprising chemist of this city, to make a number of experiments to ascertain the *principle* on which its medicinal properties depend. After a number of tedious processes he was at length so fortunate as to obtain an *alkaline principle*, to which he has given the name of *Cornine*, in conformity to the usual nomenclature; this basis he combines with sulphuric acid, and hence forms a sulphate of Cornine. As Mr. C. is not yet prepared to give publicity to the method of obtaining this salt, I shall merely state such of its physical and medical properties as have come under my own observation.

The Sulphate of Cornine is of a grayish-white colour, and intensely bitter; it deliquesces when exposed to moisture, and is about three-fourths soluble in water. In these respects, and perhaps in most others, it resembles the Sulphate of Quinine. The bark of the *Cornus Florida* yields this salt, in much the same proportion as the Carthagen bark of the shops yields the Quinine; but Mr. C. anticipates a greater proportion from the *bark of the root*, on which he is now experimenting.

Soon after Mr. Carpenter had obtained the Cornine in June last, he gave me fifteen grains for trial. I was at that time called to a case of quotidian intermittent, which had attacked the patient for eight preceding days; the intermission did not exceed eight hours, in which time I directed him to take every hour a pill containing a grain of the sulphate of cornine; he did so, and escaped the expected paroxysm, nor has he from that time to the present had any return of it.

The second case was the son of Mrs. W. aged six years. This boy had been daily attacked by chills and fevers for more than a fortnight previous to my seeing him. I directed the sulphate of quinine, in the usual doses, through several intermissions, but without effect; I then increased it to a grain and a half every hour, but it still seemed to have no control over the paroxysms, and I despaired of effecting any good by its further use. I then called on Mr. Carpenter for some of the sulphate of cornine, of which he gave me eight grains, being all that remained of the medicine. The patient took a grain of it every hour, and for the first time during his illness the paroxysm was completely suspended, and a perfect intermission of fifty-two hours obtained: the fever returned at the expiration of that time,

though in a much milder form, and assumed the tertian type: being unable to obtain any more of the cornine, I now gave the sulphate of quinine in doses of two grains every hour, since which there has been no return of the disease, though six weeks have elapsed.

I have no doubt, that if this patient could have taken twelve or fourteen grains of cornine in place of the eight, the cure would have been complete; but the dose was inadequate to the duration and violence of the paroxysms.

I have given these two cases somewhat circumstantially because they were the first in which I had an opportunity of trying the medicine. More recently, however, Mr. Carpenter, has made several drachms of the salt, and I have had an opportunity of using it in three other cases with unequivocal success; two of these cases were quotidian intermittent, the other a tertian, in all which the paroxysms have been so promptly suspended, that I have no hesitation in expressing my firm conviction of the febrifuge powers of the cornine.

The *Materia Medica* of the United States is therefore indebted to Mr. Carpenter for a new and valuable discovery, which promises to be of permanent advantage in medical practice. The cornine does not appear to be in any respect inferior to the quinine, and will doubtless prove an efficient substitute for the latter. It has, moreover, the advantage of being obtained from an indigenous plant, and the material for its preparation may be at any time obtained from our own forests, and at a moderate expense.

P. S. Since writing this paper, my friend, Dr. R. COATES, has favoured me with the particulars of a case in his practice, which fully corroborates my own experience with respect to the sulphate of cornine. Dr. C.'s patient was attacked with a remittent fever which assumed a typhoid form; on recovering from the latter, and while the system was in a very debilitated condition, he was attacked by an intermittent. The paroxysms were unusually obstinate, and continued for several weeks, sometimes in a tertian and sometimes in a quotidian form. The usual febrifuge medicines were used to no purpose, and quinine was one of the number. Dr. C. considered this an interesting case for the trial of the *cornine*, of which he procured ten grains, and gave it in grain doses; and from that time to the present, a

period of twenty days, the patient has had no return of the paroxysms.

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The observations made and published in Paris by Dr. GERDY, relative to the muscular sheaths, a year subsequent to the publication of similar researches in this city, is sufficient proof that such investigations are considered as novelties possessing some interest, even in the school which derives its principal glory from BICHAT. Those who imagine this great anatomist to have exhausted the study of the membranes by his invaluable inquiries, take a very inaccurate view of his observations, and display an extremely limited acquaintance with minute anatomy; such as cannot distinguish between a general enunciation that the fibrous membranes are continuous at certain points, and a description of the manner in which they are continued and related, do not exercise much discrimination. The observations of BRESCHET relative to the fascia superficialis, and the dissections of one of the most experienced and respected anatomists on this side of the Atlantic, so satisfactorily confirm the investigations published in this Journal by Dr. GODMAN, that he intends prosecuting his original design of making, with the assistance of several anatomical friends, a more extensive collection of materials for a systematic treatise on the subject of the fibrous membranes. The reader who wishes to know what has been said relative to the formation of the pericardium by the *pleura*, will find in BELL's and in FYFE's Anatomy,\* besides in several other authors, enough to satisfy his curiosity. For the description and figure of the muscle of the thyroid gland, (two instances of which have been procured within the last month,) he may refer to DUVERNEY's fourth plate and text, and to CALDANI's 116th plate, fig. 2; and for a fuller description, to SÆMMERING's second volume, under the title "*Musculus glandulæ thyroideæ*."

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The extract from Dr. WILTBANK's observations on the action of the heart, quoted inadvertently from "Anderson's Quarterly," in our respected cotemporary the New York Medical and Phy-

\* 8vo. Ed. in 4 vols.

sical Journal, is from a paper originally published in this Journal last winter.

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*Medical Society of the Valley of Virginia.*

At a meeting of a number of the physicians of Winchester and its vicinity, in May last, for the purpose of forming a Medical Society, it was unanimously

*Resolved*, that such a society be formed, and that it be denominated "The Medical Society of the Valley of Virginia." The society then adjourned, and met again on the 6th of June, adopted a constitution for its government, and elected by ballot the following officers for the ensuing year:—

*President,*

JOHN E. COOKE, M. D.

*Vice-Presidents,*

ROBERT T. BALDWIN, M. D.

TALIAFERO STRIBLING, M. D.

*Recording and Corresponding Secretary,*

JAMES W. THOMSON, M. D.

*Treasurer,*

HUGH H. M'GUIRE, M. D.

*Librarian,*

ARCHIBALD S. BALDWIN, M. D.

*Orator,*

JAMES W. THOMSON, M. D.

The following circular, addressed to the physicians of the Valley, by order of the Society, at the August meeting, presents a correct and general view of the important objects of its formation.

*Circular.*

At a meeting of the Medical Society of the Valley of Virginia, in Winchester, on the first day of this month, it having been represented that several physicians of the Valley had sought, through the medium of the public prints, and otherwise, information respecting the object of the society, it was unanimously resolved, that the president and secretary publish a statement on the subject, for the satisfaction of those who feel solicitous for the improvement and welfare of the profession.

Several physicians of the Valley, desirous of associating for the purpose of mutual improvement, &c. invited the attention

of the physicians of the Valley to the subject, by notice in the public papers, in the month of May last, and requested a meeting on the 6th June, at Winchester. On that day a number assembled, organized the society and adopted rules for its government. The objects of the association are, the investigation of the nature and proper treatment of various diseases of the human race, and of other subjects agitated among medical men. The collection of scarce and valuable books, plates, &c. which individually they can scarce hope ever to obtain, the regulation of their medical intercourse, &c. An essay is to be read at every meeting, and the subject discussed by those who feel disposed, whence will arise an interchange of opinion, and modes of practice, whereby it is believed, that all will improve, and those especially who live in comparative retirement will have old impressions revived, their minds stimulated to the recollection of truths fast fading away, and to the acquisition of new truths, offered in such abundance to the medical world, as to make the present time an era in the sciences, whereby their ability to contend successfully with disease, will be increased, and the public be greatly benefited. As the object is mutual improvement, that the benefits of the institution may be extended as far as possible, no other qualification for membership is required than "good moral character, respectable qualifications, and conduct." With the same view, medical students are received as junior members, with the privilege of reading essays, if they please, but there is no obligation on them to do so. The members are to pay five dollars entrance, and three dollars annually—the junior members two dollars entrance, and nothing afterwards.

The regular meetings of the society are on the Monday, before the first Tuesday, in the months of March, April, June, August, October, November, and December, at 3 o'clock in the afternoon, in the Common Council Hall, in Winchester.

## EXTRA LIMITES.

## THE PLAGUE.

*Paris Royal Academy of Sciences—Section of Medicine.*

At a Sitting of this Institution in September, Mr. Lassis presented a Memorial to the Academy, stating his reasons for considering the Plague and Yellow Fever not contagious. The subject having been already before the public, we shall state but in a very general manner the assertions which it contains:—

“Ancient Rome, (said M. Lassis,) like France, was unknown to America and to the East, but nevertheless they both suffered from the yellow fever and the plague. Whenever an epidemical disease exists, it is produced by local causes, which can always be explained. The yellow fever is nothing but a very intense fever, accompanied by the jaundice—the plague is nothing but an intense fever, attended with buboes, parotides, &c. These pestilential buboes are absolutely the same as the pustules of the nature of carbuncles which we notice in all parts of France, and which are in a manner epidemic in some of the provinces. The disorders called yellow fever and plague appear continually in all parts of France, but as they are called by another name, no mention is made of contagion and no precaution is taken. It is for this reason that they are not attended with the fatal consequences which always follow the official declaration of these maladies. The measures which are called *sanitary*, occasion all the accidents which they profess to prevent. At Cairo, as at Constantinople, when the plague has a contagious influence, it ravages only certain quarters, whilst others more healthy, are always exempt from it. The plague which exists in Lower Egypt never penetrates into the Upper.”

M. Lassis then made some observations relative to the late epidemic at Barcelona. A committee was formed, which declared unanimously that contagion was but a chimera; this committee published the reasons upon which they had formed this

opinion, which were so conclusive, that they effected the conversion of a celebrated physician, who had before been at the head of the contagionists. M. Lassis presented a list of ten communes in Barcelona, inclosed within the *cordon sanitaire*, in which three hundred sick took refuge. The inhabitants communicated constantly with the city, but no alteration took place in their health. M. Lassis concluded by declaring that he not only did not believe that either the plague, the yellow fever, or any other typhus, was contagious, but he denied also the existence of *infection*, and he thought that the adoption of this latter opinion did not cause less calamitous results than the idea of contagion itself.

At the conclusion of the memoir, of which we have given an imperfect sketch, a very interesting discussion took place in the Academy relative to contagion. M. Laennec commenced the discussion, and interrogated the author as to whether he absolutely denied the existence of contagion in every species of disease, without restriction.

M. Lassis replied, that he always excepted the measles and syphilis, but he excepted those only.

“Since M. Lassis, (continued M. Laennec,) assimilates the malignant pustule of Burgundy to the pestilential buboes of the plague, and as he pretends that these two disorders are the same, would it not be better, according to him, to begin by making experiments on the disorder which we have now before us; and thus arrive at a conclusion, which, in the opinion of non-contagionists, might be legitimately extended to the plague.”

M. Lassis replied, that the malignant pustule of Burgundy is not more malignant than the plague; but that the experiments which might be made on it, could not undeceive the public, “We must strike at the root of the evil,” said he; “it is the plague of the Levant which is feared, and it is to the plague of the Levant that we must address ourselves.”

M. Geoffroy Saint Hilaire observed, that he would communicate to the Academy, the result of the observations which he had himself made in Egypt. “I have never made them public,” added he, “either in my lectures, or by means of the Journals; and though the communications which I am going to make, do not positively refer to the fundamental merits of the question, yet I think they are not entirely devoid of all interest. The

plague does not reign exclusively in Lower Egypt; it ravages sometimes the Lower and sometimes the Upper, but never both at the same time, and it is always subordinate to the influence of the overflowing of the Nile. If this river rises very high, Upper Egypt is attacked by the plague, because it is influenced by the exhalations which arise from the damp earth heated by the sun. The Lower country, on the contrary, situated beneath the place where the emanation of deleterious gases takes place, is exempt from it. The Upper country remains healthy, because it is exempt from humidity; but Lower Egypt being in its turn subject to the influence of damp and heat, is ravaged by this destructive scourge. It is, therefore, to the Nile alone that we must attribute the plague in Egypt; and to deliver that country from it, it would be necessary to take away that river. The Nile, throughout the course of the year, dominates over the health of the country, and its variations totally change the medical constitutions of the regions which it waters; and it results that these constitutions have an energy of which we have no example in this country. This energy is such, that when there is a reigning disorder, any cause of interruption to the health whatever, produces always that disorder and no other." M. Geoffroy St. Hilaire supported this position by a number of facts, among which are the following; a waterman, after violent exertion, had the imprudence, whilst in a state of perspiration, to throw himself into the river. In any other situation, he would have contracted a cold or a pleurisy. In Egypt, however, the plague being raging at the time, he died the day after committing this rash act, as was predicted by M. Geoffroy St. Hilaire, at the moment when it took place. The same thing happened to a soldier who was addicted to drunkenness, who having one day escaped from the vigilance of the persons appointed to watch him, drank to excess; his misfortune was also predicted. "It would be useless," said M. Geoffroy St. Hilaire, "to enter minutely into the detail of other facts which I could cite. I have in my remembrance five or six cases exactly similar to those I have detailed. With another constitution, the same men who died of the plague, would have contracted the ophthalmia of the country, or perhaps an affection of the intestinal canal; in short, in every month, whatever we may do, we can never contract any thing but the reigning malady; and it is possible to say with

certainly beforehand, such and such month I shall have such a disorder, and such month another."

M. Louyer-Villermey related a circumstance which appeared to him to strengthen the opinion as to the identity of the plague with the yellow fever. "One of the most enlightened men of America," he observed, "told me, that at New York, whenever the yellow fever manifested itself at the sea side, the inhabitants of the port alone were affected with it, whilst merchants and persons in easy circumstances, who lived upon higher situations, were preserved from it. Those alone who came into the plains contracted it."

A similar circumstance as to a street which traverses New York, and separates the healthy from the unhealthy part of the city, was mentioned by M. Desormeaux.

M. Bosc related the same fact, but stated that persons might descend into the port every day without contracting the contagion, but that they could not with impunity make it their usual residence, or even sleep there.

M. Laennec observed, that in spite of all that had been said, as to the climate being as powerful an agent in the production of the plague as the yellow fever, he thought there was no reason to believe that an identity existed between those two disorders. It was requisite then to divide the question, and to make isolated experiments.

The author then reverted to his first proposition, relative to the malignant pustule of Burgundy, and observed, that researches on contagion should commence with an inquiry into this.

M. Pariset said, he was astonished, that in this discussion the most notorious facts relative to the late Epidemic at Barcelona alone, should have been brought forward. It seemed to have been forgotten that Tortosa was infected by a patient who, on leaving Barcelona, took refuge there; as was also Palma; and that in the port of Mahon similar communications had caused the same disorder. A vessel from Lara finding the ports of Spain shut, put into the Lazaretto at Marseilles. This vessel had communicated with Mahon, and almost the whole of the crew perished at Marseilles. "There is not, perhaps," continued M. Pariset, "a more healthy village in the world than Carlotta. Situate on the declivity of a hill, and removed from all species of forest or stagnant water, the air there has a free

circulation, the streets being sixty feet broad; and yet this spot, so healthy and delightful, has suffered from the yellow fever, which had been brought there by patients affected with it. It has been said that the villages in the vicinity of Barcelona were exempt from yellow fever, in spite of the number of sick who had perished there; but it is certain, on the contrary, that when an infected person is assiduously taken care of, either by a parent or a friend, the person who had constantly come in close contact with him is sure to contract the disorder. It is, however, true, that the new patient would scarcely ever transmit the disorder himself, for all contagious disorders lose their force in passing from an individual, other than the one who first propagated it. Numerous facts might be cited of muleteers, who, in transporting merchandise, had brought the disorder into places where it had not before existed. These facts are notorious, and I can assure you, upon my honour, that the great majority of Spanish physicians do not in any way doubt the reality of contagion."

The President observed, that as the Academy of Medicine were to discuss the subject of contagion when the report was presented, it would be desirable to close the debate upon the subject.

M. Lassis, who was about to address the Academy before the closing was ordered, contented himself with observing, that it would not be difficult to refute the assertions of M. Pariset.

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*Transfusion of Blood.*

A poor woman, about twenty-five years of age was attended, whilst in labour, by Mr Waller, of Aldersgate-street. Nothing particular occurred during the labour, but after the birth of the child and expulsion of the placenta, the womb did not properly contract, and during the absence of the medical attendant, flooding occurred to an alarming extent. When visited by Mr. Waller, the patient's pulse, at the wrist, was scarcely perceptible, indeed, at times it could scarcely be felt; the lips and face were of a pallid or death-like hue; and, in a word, the taper of life was but faintly glimmering.

Under these circumstances it occurred to Mr. Waller that the operation of transfusion would be a measure to rescue the patient from her perilous situation.

Dr. Blundell was sent for, and upon his arrival he found the patient had somewhat rallied, in consequence of which he deemed it better to delay the performance of the operation; for, as Doctor Blundell observed to his pupils, the operation is only justifiable in extreme and otherwise desperate cases. After waiting an hour, the patient became worse, she vomited, and was exceedingly restless, which may always be regarded as a very bad symptom; the pulse at the wrist was fluttering and occasionally not to be felt, and there was that peculiar expression of countenance which can scarcely be described; it may be called "death in the face." It did not appear proper to delay the operation, which was therefore commenced as follows.—The cephalic vein of the right arm was laid bare, to the extent of about an inch, and a blunt pointed bent needle was passed under the vein, at the lower part of the opening, so as to prevent the efflux of blood. The husband of the patient, a robust, healthy young man, was now called in, and two ounces of blood were taken in a full stream, from his arm, and received in a conical glass tumbler. An opening of about one-eighth of an inch was made in the vein of the patient, and by means of a syringe or tube, the blood extracted from the husband was somewhat slowly thrown in, towards the heart. No very obvious effects were produced from this supply of vital fluid, and after a pause of one or two minutes, two other ounces of blood were thrown in; soon after this the pulse at the wrist intermitted and there was slight restlessness, or rather desire to change posture, but the same symptoms passed away in the space of two or three minutes. In consequence of these symptoms, it was deemed prudent to wait awhile; and after a lapse of ten minutes the patient was evidently rallying.

From this period the patient went on improving, and had not a single bad symptom which could be attributed to the operation—the functions of respiration, circulation, and of the chylopoetic viscera, were duly performed—the temperature of the surface of the body was of the natural standard—neither was there any subsequent affection of the sensorium, which Dr. Blundell has known to occur in some cases, after the operation of transfusion.

The syringe employed was of brass, and well tinned on the inside to the mouth of the syringe; a pipe was fixed, of about

two inches in length, of the size of a crow's quill, shaped like a pen at the end, but with a blunt point.

Before the blood was thrown into the vein of the patient, all air was carefully expelled from the syringe, by placing the mouth upwards, and pushing up the piston until the blood appeared at the end of the tube attached to the syringe.

Dr. Blundell observed, this case demonstrated, beyond all cavil, that the blood of a man may be injected, by means of a syringe, into the veins of a woman exceedingly reduced from hæmorrhage, without causing death. Whether the syncope which occurred after the injection of the blood was the result of the operation, or of the previous hæmorrhage, may be disputed; and admitting the syncope to be the result of transfusion, we should be no more justified in rejecting the operation on this account, than in refusing to employ the lancet because it occasionally produces syncope.

As only four ounces of blood were injected, Dr. Blundell admitted, that it might fairly be questioned by some, whether the supply of so small a quantity of blood really saved the patient. The Doctor, however, (and he has seen a great deal of hæmorrhage,) is decidedly of opinion, that this timely supply of vital fluid turned the scale in the patient's favour, and rescued her from death.

[This case was related by Dr. Blundell in one of his lectures.\*]

• Thirty years ago, the experiment of transfusion of blood, under precisely the same circumstances as above, was performed by Dr. Physic.—*Editors.*



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THE

# Philadelphia Journal

OF THE

## MEDICAL AND PHYSICAL

## SCIENCES.

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ART. I. *On Prolapsus Uteri.* By JOHN T. SHARPLESS, M. D.

THIS is a disease that has long claimed the attention of medical men, and has been generally esteemed of importance, not only because it renders the afflicted female miserable by producing much actual suffering, but in process of time endangers her very existence, by inducing or increasing other complaints that may ultimately terminate in death. Though this disease has been described so accurately by many authors that it would seem almost impossible to mistake or overlook it, yet it is often the unsuspected cause of much distress, and the producer of formidable symptoms indicative of other complaints that have no existence. Although it never, so far as I am acquainted, entirely loses its own proper and distinguishing characters, yet the apparently more important complaints that it calls into being, eclipses its peculiar diagnostics, and diverts the attention from the substance to the shadow.

It is not my intention to give a perfect history of this disease, but merely to give it a little more importance than it has generally received, considering it as too much neglected

by many physicians of the day, who seem to regard it as an uncommon complaint, and self-evident when it really presents.

Females will seldom willingly draw the attention of their physicians to symptoms whose seat is in the neighbourhood of parts concerned in this disease. They may casually speak of pain in the back, and many disagreeable feelings through the body, but will mislead by assigning cold or fatigue as the cause. But when the malady has existed for any length of time and is severe, you will find them directing your attention to a constant pain in the head and giddiness, pallidness of skin and emaciation, loss of appetite, furred tongue, nausea, incessant languor and depression of spirits, they feel nervous, and may even have hysterical convulsions, frequent fever, pain in the breast, and a dry cough, oppression at the præcordia, unpleasant sensations and tenderness of the side, particularly the right, and, indeed, most of the appearances of a serious disease of the liver. This will be the amount of their voluntary description, and I have known several respectable physicians who have treated their patients actively for hepatic and thoracic derangements, whose inquiries had never led them to suspect other disorders.

A more accurate and resolute examination will lead the patient to define symptoms she had suffered for years without her imagining for an instant they were connected with real disease. She will tell you she has had a constant dragging pain in the back and pelvis as though some heavy body wished to escape from her, great tenderness of the iliac and pubic regions, frequent swelling of the abdomen, an insupportable disposition to strain, without avail, to pass water and fæces. She is subject to piles, and often great swelling, tenderness, and itching of the labia. In conjunction with these, there is much leucorrhœal discharge, which the woman often supposes is the origin of all her complaints. There is also pain shooting down from the groin, and tumefaction of the lower limbs. The catamenia is almost universally deranged both in time and quantity, producing in some cases menorrhagia, and very often an almost total cessation; and the ap-

proach of this period is always dreaded by the sufferer as aggravating all her complaints.

These are the symptoms of severe and protracted cases of prolapsus uteri; and a persevering examination will often detect the majority.\* Dr. DEWEES in an excellent article on the subject in his *Midwifery* remarks, that "the symptoms are intense in proportion to the extent of the displacement." Dr. MANSFIELD CLARKE, in opposition to this, says, "The number and violence of the symptoms are by no means proportioned to the degree of the descent of the uterus." Though my judgment and experience is but a point in comparison with the former gentleman, I must be allowed to say, that in many cases my observations have realized the latter opinion, and that when the os tinæ has been visible externally the symptoms were often less aggravated than in others where there was little sensible depression. In a nurse of this city, I found the uterus actually laying half disclosed on the chair on which she sat. How many very uncomfortable feelings were undoubtedly produced, but infinitely less than in others where an examination alone would never have detected the disease. In one instance, the ligaments are still violently on the stretch, and the parts below unaccustomed to the increasing pressure; in the other, the ligaments are

\* By some accident, I had never seen or heard of a report of some cases of this disease, by Dr. Dewees, in the November number of this *Journal* for 1824, till they were referred to by Dr. Perrine, in the last number, by which time this essay was prepared for publication. I was happy to find that my sentiments, which I had thought new, of the importance of this complaint in the production of other serious symptoms, were coincided with by that gentleman; particularly the induction of apparent disease of the liver or spleen. Notwithstanding the editor of the *Medical Recorder* declares, that "Dr. Dewees's paper contains nothing new, and that his detail of symptoms can be found in Burns's *Midwifery*," I will take the liberty of asserting, that no author that I can find in the large medical libraries of this city, has said a word about the symptoms that his paper was particularly intended to describe, I allude to the pain of the left side. My experience has found this complaint in both sides; but more particularly in the right, but the same description of suffering existed. I have also detailed some pectoral complaints, which have almost always been associated with this apparent lateral disease.

completely relaxed, and the weight of the viscus is supported by the perinæum and other parts less intimately connected with the upper belly.

In addition to the symptoms already described that we receive from the patient herself, the tactus eruditus will often discover actual and sensible displacement, assuming in different cases every grade of descent described by authors. But our touch will frequently develope nothing but a tenderness and thickening of the os uteri, without the least apparent change in the situation of the organ. This may possibly mislead, but I have known these cases as effectually relieved by the instrument as any other. How this tenderness and enlargement of the mouth and neck of the womb is produced, cannot perhaps be ascertained, but it generally accompanies this complaint, and there is no doubt many cases of simple prolapsus have been mistaken for scirrhus, and mere palliations for that disease have been used. Occasionally, the mouth will not occupy the centre of the vagina, but is thrown forwards or laterally, or, as is most common, backwards against the sacrum, producing sometimes almost an antiverision. This obliquity may depend on an inequality in the relaxation of the ligaments, or other support.

How this disease interferes so much with parts apparently little connected with the viscus concerned, is perhaps explained in an anonymous work, published in 1824, entitled, *Le Catéchisme de la Médecine Physiologique*. The author says, "La première influence de l'uterus irrité s'exerce sur les viscères du bas-ventre et de la poitrine; la seconde, agissant d'abord sur le cerveau retentit dans les nerfs locomoteurs... Ces phénomènes nerveux sont produits de la manière suivante." After describing the sympathetic nerve, and that it consists of many ganglions, he continues, "Les cordons qui sont dans l'uterus," and I add in its neighbourhood, "transmettent l'irritation de cet organe à tout le reste des nerfs, et font participer tous les viscères à la souffrance de l'un d'entre eux."

The causes of prolapsus uteri I need not mention, they have often been described, and in the list we will find leu-

corrhœal discharges assume a conspicuous place. This may possibly be sometimes true, but less frequently than usually imagined: for in continued irritations of any kind, the parts affected are generally enlarged and thickened. We know this to be the case in old discharges from the vagina, where the passage becomes hard and callous, and we might suppose better qualified from its increased size and strength, to support the superincumbent weight. Such drains are weakening to the system, but seldom to the points of emission. This reasoning is supported by the fact, that women with prolapsus, generally remark that the discharge was coeval with the other symptoms. Tumours in the abdomen may cause this descent, and I have the care of a female, whom Dr. ATLEE, Sen. has visited at my request, in whom an enormous tumour in the left side of the belly, whether of the kidney or other part, could not be told, had pressed down the uterus so much from its place, that until the size of the enlarged mass was reduced by medical means, every plan I could use failed in supporting the viscus.

Of all the modes of cure I am acquainted with, the pessary seems alone entitled to respect. Other means are often employed, and as assisting agents cannot be dispensed with, but to depend on them entirely is preposterous. Among these, are injections of astringent barks, bandages to the perinæum, confinement to the back, &c.; but the instrument must at last be resorted to, and if it does not always cure, it generally proves a blessing the suffering female can hardly exist without. In recent cases a perfect cure may be expected in a few months, but where it has existed for a long time, say years, though the means proposed may apparently so entirely remove the disease as to supersede their employment for a time, slight causes will reproduce it, and require a repetition of the practice.

From a general acquaintance with the opinions of the physicians of this city, and from my own experience, which has not been very limited, I have been led to esteem the oval pessary by far the best, as pressing least on the urethra and rectum. This is the decided sentiment of Dr. JAMES. Dr.

PHYSICK prefers the globular, and Dr. DEWEES the circular. The oval is rather the most difficult in its correct placement, but when properly adjusted of more efficacy, from its allowable larger size. Silver is the substance generally recommended for the construction of this instrument, but it is so smooth and hard, that when it does not fit very well, it will slip about without control.

The gum elastic does not possess this disadvantage, but it becomes so rough from the acrid secretion, that its use is soon precluded. How glass, ivory, or even the inflated bladder of AITKENS would answer, I cannot say; I am unacquainted with their merits. The article I have most frequently employed is cork, covered with wax till the wood cannot be seen. This possesses more merits than any other substance I am familiar with. It is, in the first place, much cheaper, as the shape and size of the instrument often requires changing during the progress of cure. There is also a tenacity in the wax, that enables it to remain in its situation better than the silver, and by the frequent injections which are always necessary for the sake of cleanliness, it remains sweet equally long. As in the adjustment of the oval pessary the long diameter must be placed from side to side, the narrow meatus of some females prevents the introduction of more than one finger to fix the instrument. When the silver or gum elastic is employed, it is utterly impossible with one finger to move the pessary on its axis, if this be necessary rightly to dispose it; but with the cork the nail of the finger forced into the wax, has complete command. The right size of this article must be ascertained by trial, great difference existing as to capacity, and from the entire failure of some particular caliber, persons have been induced to suppose this disease did not exist. From two to three inches in the long, and from one and a half to two and a half in the short diameter, is the range suiting different cases. A general cause of failure in their use, has arisen from the small size of the pessary, some women requiring a rapid increase during the subsidence of the swelling and inflammation from the uterine descent. The apparently enormous magnitude that some demand to maintain the

womb in its right situation, is necessary to perfect the cure, for whilst there is the least displacement remaining, the ligaments cannot recover their tonic contraction; and then by gradually diminishing the size, the vagina will assume its correct diameter to assist in the support. Where this great magnitude interferes from anterior or posterior pressure, wide notches must be made in the sides, producing something the shape of an 8, to accommodate the parts; and indeed the painful feelings produced by the instrument on the diseased urethra that is sometimes found from long-continued prolapsus, and also its frequent displacement by being pushed down by large and hardened fæces, would seem to demand this variety of shape in many cases. With just deference to the superior experience and judgment of Dr. Dewees, I think the aperture in his instrument is by far too small. It ought to admit the finger, which is necessary in the removal, as the least painful means of turning it on its edge and extracting it, and also for the purpose of ascertaining and correcting the position of the os tinæ, which can seldom be done over the edge of the pessary as he recommends. I suspect also, that his minute orifice would soon become closed by the tenacious secretion of the parts, and prevent the passage of the catamenia. This gentleman, I understand, has mentioned in his lectures, that the cervex uteri has sometimes got so far through a large orifice as almost to strangulate. This effect I have never known, but as his sentiments on any subject connected with medicine are entitled to respect, this should be attended to and guarded against. A well covered cork pessary need be removed but once in two or three months, and when absent, the patient should never rise from a recumbent position, for a few minutes uprightness will undo all the improvement of weeks.

From the notes I have taken of many cases, I have selected one or two illustrating the influence of this disease on the constitution.

P. R. a young unmarried woman, aged 19 years, living in Chesnut street. She had been subject for three years to a hard, dry cough, much pain and tenderness of the right side, so that she could not lay on it; a constant tightness and pain

of the præcordia, feeling as though "her back and stomach were tied firmly together." Constant suffering in the head had rendered her eyes so irritable that fine work had been long precluded. Her size and strength had been so much reduced by fever and night sweats, that her friends had advised her to prepare for dissolution. These complaints were associated with almost incessant nausea, frequent chills, swelling and soreness of the abdomen and lower limbs. She had also the usual symptoms about the back and pelvis, with great discharge, inflammation and obstructed catamenia. She had been under the care of a respectable physician in the country, who had treated her for ague and fever, by bark and wine; another had cupped, blistered, and profusely salivated for hepatitis and disease in the head; a third had made dropsy her complaint; a fourth had given active emenagogues, and a fifth had made her lungs the target for his medical archery. The Protean Hydra, however, remained undestroyed, or even affected, and from a constant increase of disease and danger, she had calmly given up all hopes of life. From the long continuance of her numerous complaints, I feared disease might have been actually excited in other parts of her system.

She was directed to lay in bed for a few days, a dose of salts was ordered, and a blister to the pubic region to remove the disposition to inflammation that had existed for several months. In a short time a pessary was introduced, and she returned to her usual employments. I did not give a single dose of other medicine to observe what connection existed between her many complaints. In six weeks her cough, fever, and pain had entirely ceased, her flesh, colour and strength were returning, and from a state apparently on the verge of the grave, in four months she called on me to return the instrument, saying she enjoyed as good health as ever, and wished to know if it would be proper for her to get married.

Another single female, aged 25, had been treated by two respectable medical men of this city, the one for rheumatism in the breast, the other for dysmenorrhœa. She had also much cough, great pain in the left side shooting through to the back, constant pain in the head, and swelling of the abdo-

men and lower limbs. In this case a recumbent position did not the least mitigate the symptoms either of the pelvis or other parts. I advised her of the seat of her real disease, and warned of the bad effects of violent exercise whilst the womb remained unsupported, which operation she had refused to submit to. She went, however, to the country, and riding and other active amusements produced such a descent of the part, that an extensive and dangerous inflammation was the consequence, affecting even the abdomen and thighs, and running so high that for two days it was impossible to remove her urine even by a catheter.

In this case more than eight months of active and judicious treatment, and perfect confinement to bed, by a respectable physician in the country, was necessary to prepare the parts for the pessary. When she returned to the city, which was more than one year from the commencement of her confinement, she could sit up but for a few hours at a time, from debility, fever, and great pain of the parts.

An examination developed much disease of the neck of the bladder and whole length of the urethra, from the long unnatural pressure of the uterus, and constant inflammation. The shape of the pessary was instantly changed as before proposed with the notch, and the parts were perfectly accommodated. The pain and fever has now nearly disappeared, she is regaining the bloom and flesh of health, and though but three months have elapsed since her return to the city, a ride of several hours leaves not a pain behind.

Many similar cases to these I could relate, but sufficient has been said to prove the importance of the great first cause, and, indeed, I have so frequently detected this uterine derangement as the real origin of so many serious local, as well as constitutional symptoms, apparently unconnected with that part, that when I meet with patients who seem to be afflicted with so many diseases, I am constantly led to inquire for this complaint, and have been astonished to find the mere displacement, or its disposition in that viscus, possessing such a sovereign influence over the whole system.

ART. II. *Analysis of the Waters of the Bedford Mineral Springs, &c.* By WILLIAM CHURCH, M. D. of Pittsburgh. Read before the Pittsburgh Medical Society.\*

THE borough of Bedford is a neat, thriving and healthy place, containing about one thousand inhabitants. It is the seat of Justice for Bedford county, Pennsylvania, and is situated on the great Western Turnpike, which passes through Pennsylvania, from Philadelphia to Pittsburgh, a few miles east of the chief elevation of the Alleghany mountains.—The Springs, the Chalybeate excepted, are situated about one and a half miles south of Bedford, in Shover's valley, which lies between Constitution hill, on the east, and Federal hill, on the west. The valley is watered by Shover's creek, which passes through it, and discharges itself into the Raystown Branch of the Juniata river, about a mile east of the town.

The principal Springs are *Anderson's*, or the Main Spring; *Fletcher's*, or the Upper Spring; the *Limestone* Spring; the *Sweet* Springs; the *Sulphur* Spring, and the *Chalybeate* Spring.

Anderson's, or the principal Spring, issues, in a very copious stream from a fissure in a limestone rock, on the west side of Constitution hill, about thirty feet above the level of Shover's creek, and is situated within twenty yards of the verge of the bank of the creek. The water is clear, lively and sparkling. At ten, A. M. on the 28th of May, 1825, the temperature of the water, in the Spring, was fifty-eight degrees of Fahrenheit, while that of the surrounding atmosphere was seventy degrees of the same scale. Its specific gravity is 1029. It has a peculiar saline taste, resembling a weak solution of epsom salts in water, impregnated with carbonic acid, and exhales no perceptible odour. On exposure in an open vessel to the air, it becomes vapid; but does not become turbid, or deposit a sediment. The water deposits carbonate of iron on those substances over which it constantly flows.

\* This analysis was made at the Springs on the 27th and 28th of May, 1825.

Limestone, iron ore, calcareous and siliceous substances abound about the Spring.

The water was examined by the following tests or reagents:—

*Experiment 1st.* It permanently reddened the infusion of litmus.

*2d.* It permanently reddened litmus paper.

*3d.* The water changed the infusion of turmeric to a reddish brown.

*4th.* To turmeric paper it gave a light brownish shade.

*5th.* Lime water gave a white precipitate, soluble with effervescence in muriatic acid.

*6th.* Barytes water gave a copious white precipitate.

*7th.* One drop of the solution of the muriate of barytes, added to one ounce of the water, instantly caused a milky appearance, and a white precipitate soon fell to the bottom of the glass.

*8th.* Nitrate of mercury caused small white flocculi to appear in the mixture, and, on standing, gave a whitish precipitate.

*9th.* Nitrate of silver instantly produced white clouds, and on standing, gave a darkish white precipitate; after the water was poured off, and diluted sulphuric acid dropped on the precipitate, muriatic acid gas was evidently evolved.

*10th.* Solution of soap in alcohol became very turbid.

*11th.* One ounce of the water, added to two ounces of alcohol, immediately gave the mixture a beautiful milky appearance, and, on standing, a very copious, curdy, white precipitate was deposited.

*12th.* Oxalic acid instantly caused a copious white precipitate to fall down.

*13th.* Caustic potash gave a copious white precipitate.

*14th.* Pure ammonia gave a white precipitate.

*15th.* Solution of the fully saturated carbonate of ammonia produced no change; but on adding a solution of the phosphate of soda to the mixture, a copious white precipitate was given.

16th. Tincture of galls slightly darkened the colour.

17th. Gallic acid caused the water to assume a light purplish brown appearance, and, on standing, gave a darkish white precipitate.

18th. The solution of the super-acetate of lead gave a copious white insoluble precipitate.

19th. Characters traced with the solution of the acetate of lead on unsized paper, exposed to the vapour of the heated water, remained unchanged.

20th. and 21st. Solutions of arsenic and sulphate of copper produced no sensible effect.

22d. Muriate of lime caused a slight turbid appearance, and, after standing a few hours, a minute white precipitate was deposited.

23d. By boiling in a glass vessel, a white precipitate was given.

24th. Lime water, gallic acid, and tincture of galls effected no changes on the boiled water.

25th. Oxalic acid, litmus, caustic potash, carbonate of ammonia, and phosphate of soda added after each other; nitrate of silver, muriate of barytes, produced the same changes on the boiled water, which they did before it was boiled.

#### RECAPITULATION.

The two first experiments indicated the presence of a mineral acid.

Experiments 3d and 4th, showed the existence of an alkali or alkaline earth. The 5th experiment proved the presence of carbonic acid.

Experiments 6th and 7th, indicated the presence of sulphuric acid; and the 8th experiment proved the existence of mineral acid.

Experiment 9th, showed the presence of muriate of soda. The presence of earthy substances is inferred from the 10th experiment; of salts, formed by the union of sulphuric acid with an alkaline earthy base, is inferred from the 11th experiment.

The 12th experiment proved the existence of lime; and

the 13th, 14th, and 15th experiments, indicated the presence of magnesia.

Experiment 16th proved the existence of iron, which was confirmed by the 17th experiment. This experiment also showed, according to Wehsumb, the presence of an alkaline earthy salt.

The precipitate given by the 18th experiment, was the sulphate and muriate of lead; and it is thence inferred that the water contains sulphuric and muriatic acids.

Experiments 19th, 20th, and 21st, prove the non-existence of sulphuretted hydrogen gas.

The 22d experiment indicated the presence of an alkali.

An earthy substance was precipitated by boiling, in experiment 23d; and the lime water, tincture of galls, and gallic acid, effecting no changes on the boiled water, in experiment 24th, proved that the iron was held in solution by a volatile and not a fixed acid; for on the application of heat, the carbonic acid was expelled, and the iron precipitated.

The 25th experiment proved, 1st, the existence of sulphuric and muriatic acid—2d, that the solvents of lime, magnesia, and soda, were the mineral acids.

A quart of water, evaporated to dryness, gave *thirty-one* grains of a residuum. The same quantity of water, treated agreeably to the rule laid down by Wehsumb, contained eighteen and a half cubic inches of carbonic acid gas. The residuum, treated according to the rules given by Dr. Henry in his System of Chemistry, gave the following result:—

Sulphate of magnesia or epsom salts,	20 grains.
Sulphate of lime, - - - -	3½ do.
Muriate of soda, - - - -	2½ do.
Muriate of lime, - - - -	¾ of a grain.
Carbonate of iron, - - - -	1¼ grains.
Carbonate of lime, - - - -	2 do.
Loss, - - - - -	¾ of a grain.

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31 grains.

To which must be added eighteen and a half cubic inches of carbonic acid gas.

*Fletcher's, or the Upper Spring,*

Issues from a fissure in a limestone rock, on the west side of Constitution hill, about one hundred and fifty yards south of Anderson's Spring. It is also a copious Spring. At ten, A. M. on the 28th of May, 1825, the temperature of the water in the Spring was fifty-five degrees of Fahrenheit's thermometer, while that of the surrounding atmosphere was seventy degrees of the same scale. The foregoing experiments, made on this water, gave rather more iron and common salt, less magnesia, and about the same proportion of the other substances: and the general observations made regarding Anderson's Spring, are equally applicable to this Spring.

It gives me great pleasure to state, that the foregoing analysis of these waters, confirms that made a few years ago by "one of the first chemists of the nation."\* It furnished, by this analysis, 1st, Carbonate of lime with excess of acid—2nd, sulphate of magnesia, or epsom salts—3rd, sulphate of lime, small quantity—4th, muriate of magnesia—5th, carbonated oxide of iron. Regarding this analysis, I would respectfully observe, that as the water was sent in bottles to Baltimore, and there analysed, Dr. De Butts laboured under many disadvantages. This gentleman, in relation to the salts of these waters, observes "that they are rich," and adds, "I feel much interested on this subject, as I think that the Bedford waters are entitled to stand high in the public estimation."

The Board of Managers of the Springs, in their circular letter, very justly observe, that "from the results given, it is plain that those waters must be laxative and tonic, and experience has amply proved that they possess these effects in a high degree. They will also excite full purging; and when the first passages are charged with bilious or other acrid matters, the water will remove them, with as much activity as the most powerful purgatives. The water also excites the action of the kidneys and skin, causing a very liberal secretion of urine and perspiration."

They also state, that "the intimate connexion subsisting

\* Dr. De Butts of Baltimore.

between the stomach and bowels, and other portions of the animal body, gives to laxatives and purgatives a very extensive influence in the practice of the medical profession. The alimentary canal being considered the centre of sympathy, all impressions made there, readily communicate themselves to the most distant parts of the system, and effects are, in consequence, produced, much more important than the simple act of purging."

"In chronic diseases, those only in which mineral waters have salutary powers, the morbid sympathies are often so complicated as to baffle the best efforts of the physician. A long course of purgatives is disgusting, the patient becomes inattentive, and his taste and feelings conspire to render the wisest plans ineffectual. Mineral waters, change of air, and exercise, are, therefore, applied with the most pleasing results."

"The sulphates and muriates of magnesia, with which the Bedford water is highly charged, are decidedly purgative, while the carbonates of iron and lime are as decidedly tonic." To which I would add, that the muriate of soda, being a promoter of digestion, and the carbonate of soda, from its mild, laxative and antacid nature, will essentially contribute to the efficacy of the water. This combination of purgative and tonic powers, forming the basis of this mineral water, happily imparts, no unpleasant taste to the palates of most persons; and all, after a short experience, find the impression agreeable."

This respectable body further state, in their circular, that "the Bedford waters, drunk with proper precaution, respecting quantity, temperature, diet and exercise, accompanied with the judicious use of the baths, are found to be salutary in most states of chronic disease. In hepatic affections, in diseases of the stomach and intestines, in dyspeptic and hypochondriacal derangements, in hæmorrhoids and in all the varieties of intestinal worms, the water has effected numberless cures. In secondary diseases of the lungs, originating in the sympathies of those organs with the stomach and liver, the cures have been equally certain. This class of disease, is

marked by the general symptoms of pulmonary consumption, asthma, &c. but when these diseases have their primary seat in the lungs, these waters have been found to be useless, excepting in the forming state of primary consumption, in which they have done well. In the diseases of the skin and of the kidneys, and especially in calculous and gravelly affections, they have been very efficacious. In rheumatism of weak excitement, in anasarca and the various uterine diseases, as obstructions of the menstrual flux, its excess, the fluor albus, painful menstruation, &c. many cures have been effected, whilst its use has been generally beneficial. In diabetic and gouty complaints, it has been used with great profit. In the debility following the cure of acute diseases, or the remedies necessary to remove them, and in the weakness consequent on the cure of syphilis by mercury or otherwise, the Bedford waters have been found to be good restoratives. In all those chronic affections, which are too often the consequence of acute diseases in southern climates, and especially those of a bilious character, these waters, together with the bracing vigour of a mountain atmosphere, effect the most happy changes." And, also, "that the experience and observation of fifteen seasons, have established the above facts, on which the public may rely with confidence."

The solvents of the greater part of the substances held in solution by this water, being fixed ones, render it peculiarly fit for transportation, and it is carried away, almost daily, at a great expense, to the cities and neighbouring states, where it produces very beneficial effects.

#### *The Limestone, or Lower Spring,*

Is a very bold Spring of pure limestone water, which issues from two or three fissures in a limestone rock, on the west side of Constitution hill, about two hundred yards lower on the creek than Anderson's Spring, and about forty feet below the level of that Spring. It issues in sufficient quantities to turn an over-shot mill. On the 28th of May, 1825, the temperature of the water in the Spring, was fifty-one degrees of

Fahrenheit, and that of the surrounding atmosphere was seventy degrees of the same scale.

### *The Sulphur Spring,*

Rises on the west side of Shover's creek, about two hundred yards distant from Anderson's Spring. It is a weaker Spring than the others, and the water is covered with a thin whitish pellicle. The water exhales a very strong odour of sulphuretted hydrogen gas. Its temperature was fifty-six of Fahrenheit, while that of the atmosphere was seventy-one degrees. It has a peculiarly unpleasant hepatic taste, which I cannot well define. Chemical experiments, conducted in the same manner as those before detailed, prove, that it holds in solution carbonic acid, sulphuretted hydrogen gas, small quantities of lime, magnesia and common salt, and that it contains no *iron*.

Concerning the remedial effects of waters containing sulphuretted hydrogen gas, Dr. John Armstrong, of London, in his very valuable essay "On the Efficacy of Sulphureous Waters in Chronic Diseases," after much able reasoning on the nature of the Harrowgate and Dimsdale waters,\* whereby he satisfactorily elucidates his subject, tells us, that "sulphureous waters operate specifically, somewhat similar to mercury; that aided by moderate bleedings, laxatives, blue pills, &c. he has cured many cases of chronic disease, which, in his opinion, were incurable by other means; and that these waters *properly* exhibited, will cure chronic inflammations, wherever seated." And he also tells us, in page 65, that "he lately saw two remarkable examples of *phthisis*, which *appeared* to be cured by the Dimsdale water." It is unnecessary for me to add, that we can have no higher authority than Dr. Armstrong.

### *The Sweet Springs,*

Are two in number, and issue from fissures in slate rocks, about fifty yards apart, on the east side of Federal hill, about one hundred and fifty yards distant from Anderson's Spring,

\* These waters are highly charged with sulphuretted hydrogen.

from which they are separated by Shover's creek. They are copious Springs of remarkably pure water, which is very clear and colourless. Its temperature was, on the 28th of May last, fifty-two degrees of Fahrenheit. Infusions of litmus and turmeric, barytes, nitrate of silver, carbonate of ammonia, and phosphate of soda, added after each other, muriate of lime and tincture of galls, effected no changes on this water. Lime water rendered it slightly turbid. It is the purest water I ever saw ; and all that is necessary to render it as pure as distilled water, is, to expel the carbonic acid by boiling, after which it can be kept in well stopped bottles, and used for the same purpose as distilled water. The water of these Springs is used for cooking, washing, &c. by the residents at the Bedford Springs ; and the visitors decidedly prefer it for drinking water ; and, on account of its purity, they very appropriately called these Springs the *Sweet Springs*.

### *The Chalybeate Spring,*

Rises in a meadow, about one and a half miles north east of Bedford, and about three miles from Anderson's Spring. It is not a copious Spring. The water exhales the peculiar odour of sulphuretted hydrogen gas, and is covered with a thin whitish pellicle. When first taken from the Spring, it is clear and limpid ; but on exposure, in an open vessel, to the action of the air, it becomes turbid. Its taste is ferruginous, and slightly hepatic. Experiments conducted as those heretofore detailed, prove, that it contains carbonic acid, sulphuretted hydrogen, carbonate of iron, with muriate of soda, and a minute portion of magnesia. The Spring is surrounded by a species of iron ore, called bog ore. A singular fact, connected with the history of this Spring, is, that part of a skeleton of a mammoth was found, when cleaning it out.

"The general operation of chalybeates," says Dr. Saunders, in his 'Treatise upon the Mineral Waters of Europe,' "is to increase the power of the secretory system, in a gradual, uniform manner ; and at the same time, by the permanency of their stimulus, or some other cause, with which we are not well acquainted, to impart to the body, a gentle and salutary

increase of strength, tone, nervous energy, and general vigour of all the functions. It is therefore chiefly in those diseases that arise from slow beginnings, and are attended with great laxity and debility of the solids, but without much organic disease, that these waters are found to be particularly useful."

The position of the Springs has its peculiar advantages. They are situated in the heart of a healthy and plentiful country, abounding in picturesque and romantic scenery, interspersed with hills and vallies, which are rich in various minerals; and blessed with delightful streams of the purest water, and a fertile soil. The temperature of the summer, as well by night as by day, is cold, agreeable and highly refreshing.\*—These Springs are distant

From <i>Philadelphia</i> ,	-	195 miles.
From <i>Baltimore</i> ,	-	130
From <i>Washington City</i> ,	-	130
From <i>Pittsburg</i> ,	-	93 $\frac{1}{2}$
From <i>Wheeling</i> ,	-	140 and
From <i>Cumberland</i> , on the National road,		30 miles.

The Improvements at the Springs, are extensive and beautiful, and are annually increasing "by the appropriation of *all the yearly taxes and contributions* received from visiters using the waters."

On the east side of Federal hill, are pleasantly situated two large, airy, and convenient buildings, each two and a half stories high, and one hundred and thirty feet long. In these buildings is a large drawing room, splendidly furnished; a large dining room, in which upwards of two hundred persons, can conveniently dine at one time;† bed chambers for single persons, families, &c. and every conveniency is appended to the buildings. South of this, in the valley, is erected a large frame building, two stories high, and one hundred and forty feet long, in which the family of the resident at the Spring resides. Cooking, &c. is done in the lower story of this building. Bed chambers occupy a principal part of the upper

\* Statement of the Board of Managers.

† The public houses at the Springs and in town, can conveniently accommodate five hundred persons at one time.

story. This house has fine walks in front, and is surrounded by a neat fence. Just in front of this building is a statue of the Goddess of Health, raised on a pedestal. From the crown of her head constantly issues a stream of the spring water, which, if the day is not windy, the figure catches in a bowl. This has a fine appearance. It is surrounded by a balustrade.

Houses for the cold, shower and warm baths are erected, in which there is every accommodation for taking the baths, and an attentive and obliging bath keeper takes charge of the establishments. The water that supplies the warm bath, is conveyed from the upper spring, through a tunnel which passes under the channel of Shover's creek.—The same tunnel supplies a trough for watering horses ; and I was credibly informed, that the use of the water cured the botts, and other diseases of horses; and that noble and valuable animal becomes remarkably fond of the water after drinking it a short time.

To describe the serpentine and beautiful walks up Constitution hill ; the artificial lake of fresh water, on which small boats can pleasantly sail; the small artificial island in the lake, on which the managers intend to plant choice shubbery, and the other improvements, would extend this lengthened paper, so as to tire the reader. I shall therefore pass them without further notice ; and conclude by observing, that although I cannot say with Dr. Goldsmith in his "Deserted Village,"

" Here smiling Spring its earliest visits pays,  
" And parting Summer's lingering bloom delays,"

Yet I can with great justice say, here nature has formed the scenery truly picturesque and romantic ; and art has harmoniously combined with her in rendering it still more beautiful. Here, also, "the chemist, the geologist, the mineralogist, the botanist, the landscape painter, and the general lover of nature, will find much to employ, amuse, delight, and reward attention, or beguile the tedium of valetudinary habits and distresses:"\* and that the facilities for travelling to

\* Philadelphia Journal of the Medical and Physical Sciences, vol. 6; p. 57.

and from the cities, by turnpike roads, &c. good accommodations, scenery, climate, the efficacy of the waters, &c. all combine to render Bedford one of the principal watering places in the United States.

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ART. III. *Observations on several species and varieties of Cinchona Bark.* By GEORGE W. CARPENTER.

IN consequence of the late prevailing endemics, remittent and intermittent fevers, which have been so extensively experienced in almost every section of our country, the cinchona bark has increased very considerably in demand, and has become one of the most important articles of the *Materia Medica*; and as the descriptions of the respective species and varieties of this important genus are so obscure, ambiguous and confused, that they tend rather to involve the inquirer in more dense obscurity than to develope their true characters, and as there is no method so well calculated to ascertain the relative and comparative strength of the different species of Peruvian bark, as that of analysis, I have thought proper under these impressions to submit to trial the most important species which now occur in our commerce, by extracting the alkaline principle upon which its tonic and febrifuge virtues entirely depend, and upon the results of which their comparative strength and quality may be accurately ascertained. I have also described these species as they now occur in commerce, which, by their physical characters may be severally distinguished.

*Calisaya Bark.*

Of this very superior species of Peruvian bark there are two varieties in commerce—

1st. *Calisaya Arrollenda*, (Quill *Calisaya*.)—This variety is in quill from three-quarters of an inch to an inch and a half in diameter, and from eight inches to a foot and a half in length. The epidermis is grey and whitish on the exterior,

and of a reddish brown beneath, the peculiar features of this bark and the characters by which it may be readily distinguished are the following:—

The epidermis is thick, and is very easily removed from the bark; hence you find in the seroons or cases the greater part deprived of this inert portion, it has many deep transverse fissures running parallel, the fracture is woody and shining, the interior layer is fibrous and of a yellow colour; the taste is slightly astringent and very bitter.

This species of bark will yield a much larger proportion of the active principle, (quinine,) than any other bark in commerce, and consequently may be justly esteemed the best.

2d. *Calisaya Plancha*, (Flat *Calisaya*.)—This variety consists of flat, thick, woody pieces of a reddish brown colour, deprived of its epidermis and the interior layer more fibrous than that in the quill. This variety yields from twenty to twenty-five per cent. less quinine than the *arrol-lenda*, and is consequently a less desirable article.

#### *Superior Loxa, or Crown Bark.*

*Loxa* is the name of the province and port where this bark is obtained, and from whence it is exported. It was in this province *cinchona* was originally discovered. This bark has been highly esteemed by the royal family, and is that which has been selected for their use: hence the name of Crown Bark. This bark is in small quills, the longitudinal edges folding in upon themselves, forming a tube about the circumference of a goose quill, and from half a foot to a foot and a half in length. It is of a greyish colour on the exterior, covered with small transverse fissures or cracks, the internal surface is smooth and of an orange red; it is of a compact texture and breaks with a short clear fracture; it is the bark of the *cinchona condaminia*, and is known at *Loxa* by the name of *cascarilla fina*. Yet notwithstanding, this bark appears to have had the decided preference to all other species, analysis fully indicates that this bark is not equal in medicinal strength to that denominated *calisaya*. This bark is more astringent and less bitter than the *calisaya*.

This species yields from twenty-five to thirty per cent. less quinine than the former, the crystals of which are not so well characterized.

Experiments which I made upon the Carthagena bark, of better quality than the market generally produces, yielded about one-twelfth the quantity produced by the Calisaya arrollenda.

*Remark.*—When the calisaya bark was first introduced here it was considered an inferior article, and would not bring its cost in South America, the decision being grounded upon its external characters, but such is the deception of external appearances, that when submitted to the infallible test of experiment it was proved to be the best. Specimens of the Calisaya and Loxa barks may be procured of Charles Marshall, Druggist, of this city.

N. B. After having found the best species of Peruvian bark, by the several specific characters, one very important adventitious character yet remains to be investigated. It is a fact, established beyond controversy, that age acts very powerfully in deteriorating the active properties of bark, insomuch that the best species of Peruvian bark when old, is little superior, and sometimes even inferior to *Carthagena* when fresh, hence it is that large parcels of a superior species of Peruvian bark which would have commanded at least two dollars per pound, (when fresh,) at Cadiz, has been offered publicly in this city at twenty-five cents per pound, without a purchaser, and which, it appears, has been operated upon by no other unfavourable agent but age. In what manner age, or rather the circumstances connected with age, acts upon bark other than by a decomposition, oxidizement, or volatilization of its active principles we know not. FABRONI states that cinchona loses its solubility, and consequently its activity, by long exposure to the air, but does not state his opinion as to the manner in which it is thus affected, we cannot, however, conceive under existing circumstances how the solubility of Peruvian bark can be diminished, except through the agency of oxygen, and it is by this means the extract of bark, prepared according to the common formulas of our dispensaries are devoid of

utility, for owing to the oxidizement of the extractive matter the solubility of the extract is so diminished during its formation, that scarcely more than one-half is soluble in water.

From various experiments which I have made upon Peruvian bark in different states, I have observed as an unequivocal result, that the same species of bark which when fresh is very productive of quinine, when old will produce little or none of that active principle upon which its virtue as a medicine depends.

The prominent features which characterize old bark, and distinguish it from recent, are the following:—

Old bark has lost that bitter and astringent taste and peculiar aromatic odour, which are prominent characteristics of recent bark of good quality. The specific gravity is also sensibly decreased, and the fracture instead of being shining and compact, is dull, fibrous, and of a loose texture, and the colour very frequently passes from a bright orange to a dull brown as the bark advances in age. By attention to these few conspicuous characters, no mistake can arise in the selection of good bark, unless there is a great deficiency of judgment; yet notwithstanding the distinguishing characters of Peruvian bark in these two states, are so prominent and striking, we regret to say that gross inaccuracies have been made public by men whom we might suppose most capable of appreciating the quality, under the influence of every incidental circumstance.

Having succeeded in the preparation of sulphate of rhubarb, I shall submit my formula in the next number of this Journal, should it be deemed sufficiently interesting.

ART. IV. *Observations on Gall and Spurzheim's Theory*. By  
JOHN P. HARRISON, of Louisville, Ky.

THE system of GALL and SPURZHEIM, as set forth by Dr. Spurzheim, in the large work published in England in 1815, as illustrated by Mr. COMBE in his work on Phrenology, and as advocated by the well written essays of Drs. BELL and COATES, published in the Philadelphia Medical Journal, comes forward with high pretensions to challenge our acceptance. Originally promulgated by very able physiologists, and the best cerebral anatomists in Europe, this system has been ingeniously illustrated and upheld by the strength of argument which its supporters, both in Europe and America, have put forth in its defence. Thus illustrated by a captivating rhetoric, and enforced by the sober inferences of improved judgment, it is no wonder that it should have had such a free currency, and so wide a scope of influence.

But the hasty adoption of it, without a cautious sifting and scrutinizing of its fundamental doctrines, and the slow admission of it into the mind upon the grounds of such examination, is assuredly injurious to its progressive and ulterior advancement. It is better for a system of philosophy to fight its conquering way through a host of assailants, and win at last the proud eminence, than to glide smoothly on its career of conquest, perhaps not through its essential strength, but from the easy yielding of its opponents. If it acquire victory and renown by the slow and sure approaches of superior power, it will, like the lofty oak, in the sublime lines of Virgil—

"Immota manet, multosque per annos  
Multa virum volvens durando sæcula vincit."

But if a hasty adoption of the system take place from superficial perquisition and ready credulity, then soon "from the full meridian of its glory it will haste to its sitting."

These reflections are made to bear upon the system now under discussion, because I conceive that its advocates have

in some instances displayed too much impatience and heat of argument in opposing the attacks made on it: and, what is ever to be deprecated by every sincere inquirer after truth, an excessive vituperative spirit has glowed beneath the load of opposition, which has been, in many instances, unfairly thrown on the subject. Surely the enlightened defenders of the system have no need to resort to irrelevant reflections upon their opponents, nor endeavour, like Æneas, to escape from the field in a cloud.

As an opponent of this system of philosophy, I will strive in discussing to keep clear of the misleading influence of those feelings which darken too often the pages of the controvertist, and stain the robe of the scholar. I will discuss the subject in the order presented in Dr. Spurzheim's work.

Dr. Spurzheim designated the system the "Physiognomical system, founded on anatomical and physiological examination of the nervous system in general, and of the brain in particular," but Mr. Combe in his work, employs the more appropriate term for such a system, but perhaps less peculiarly applicable to this, the word "Phrenology," for the words *Φησι* and *λογος* mean a discourse on *mind*, therefore the system would seemingly have more to do with metaphysical arguments than it absolutely has, for the advocates of this, reject the common systems of intellectual philosophy as erroneous.

The first part of Dr. Spurzheim's work is devoted to the anatomy of the nervous system. I cannot enter into a detailed analysis of all the peculiar views set forth in the work of Dr. Spurzheim, but shall merely touch with flying hand on some points.

Three methods of dissecting the brain have been employed by anatomists.

The first mode is the one ordinarily pursued; it consists in demonstrating the brain by horizontal section, beginning at the upper part of the organ.

The second method, which is rarely practised, consists in elevating the posterior lobes of the brain; after removing the dura mater and penetrating between the *tubercula quadri-*

*gemina* and the *fornix*, the anterior pillar of the fornix is cut, the lateral parts of the hemispheres are detached, and they are turned forwards.

The third method, and the one pursued by Gall and Spurzheim, for demonstrating the organ, is superior to the other two modes.

This plan of cerebral dissection was first "sketched by Varolius, and more fully detailed by Vieussens." The under part of this complex organ by this mode is first examined, and "the *medulla oblongata* is followed across the *pons varolii*, the *thalami optici*, and *corpora striata*; its fibres are seen expanding to form the hemispheres; it is even possible to stretch out the hemispheres by removing their lateral attachment, to the *crura cerebri*; to divide longitudinally the spinal marrow, and the cerebellum; and then each half of the former is seen forming a pedicle, which is implanted into the side of the hemisphere, as the stem of a mushroom is fixed to its pileus."\*

To the labours of Drs. Gall and Spurzheim has been awarded by the common consent of the most enlightened cultivators of anatomy, the palm of superiority for their dissections and demonstrations of the brain. The following extracts will show in what respect they differ from the more commonly received opinions.

"We are of opinion," says Dr. Spurzheim, in the work referred to above, "that the nervous system must be divided and subdivided, and that each part of these divisions and subdivisions has a particular origin." p. 13. "The different parts of the brain and of the spinal marrow, the pretended cerebral nerves, and the nerves of the abdomen and thorax, have no common origin; but every part has its separate origin." p. 18. "We consider the grey substance as the matrix of the nervous fibres." p. 19. "The fibrous structure of the brain is proved by dropsy, or by boiling in oil, or by a stream

\* Report on Gall's Anatomy of the Brain, by a Committee of the French National Institute.—*New York Medical and Philosophical Jour. and Rep.* Vol. 1. No. 2.

of water directed by a syringe" on a "convolution; we thereby separate the layers one from another, we may see their fibres throughout their whole expansion." p. 20. "Moreover, when we merely scrape the white substance in the direction of the fibres, we can with the naked eye follow them into the grey substance of the convolutions; but when we scrape cross-ways or side-ways, the fibres are pulled out of their natural direction, and they visibly break off." p. 22. "The nerves in general may be divided the same way as their functions. Thus certain functions must be considered as the result of the organization alone; and others as taking place with consciousness, and being the effect of the mind. The former class of functions constitute organic or automatic life; the latter animal life." p. 23. "The brain must be considered as the organ of the moral sentiments and intellectual faculties." p. 132

The same mass of brain cannot preside over the same functions; the brain is not one single organ, but composed of as many particular and independent organs as there are particular and independent manifestations of the mind.

The configuration of the skull has been modelled by the brain; the bony mass being deposited according to the form and size of the brain. The organs are double, though as in the external senses the consciousness is single. Drs. G. and S. "maintain that they are warranted in concluding from ample data, that some particular projections of the cranium indicate an evolution of a part of the brain below; and the possession of some faculties of which it is the organ. The organs are not confined to the surface of the brain; they extend from the surface to the great swelling of the occipital hole, (medulla oblongata,) and probably to the commissures; for the whole mass of brain constitute the organs."\*

From the above extracts it will be seen that there are, in the anatomical description of the brain, as given by Drs. Gall and Spurzheim, two leading peculiarities, which have a bearing on the present discussion.

\* Cooke on Nervous Diseases, p. 42. Am. ed.

1st. That the brain is not a single organ, but is composed of as many separate organs, or distinct portions, as there are particular manifestations of intellect or moral sentiment.

2ndly. That these distinct divisions or separate portions, called organs, indicate their relative developement, by the projections and prominences on the exterior part of the skull.

In this stage of the general argument, I will make a few remarks on the above noted peculiarities of the anatomy of the brain; the physiology and physiognomy will engage our attention in consecutive order.

“The brain,” says Dr. S. “is not a single organ.”

In one sense, no organ of the body, however simple in its organization, is single. The eye, that delicately constructed and admirably adapted organ for the reception of the rays of light, is not single as regards the various parts which go to constitute its unity and individuality of function. Nor is the stomach single, only in the oneness of the purpose for which its parts were constructed. But it is urged that these separate and independent portions of the brain can be demonstrated by the hand of the skilful anatomist.

Let us receive from Dr. Spurzheim the explication he makes on this point. “The anatomy of the brain in particular, aids us in establishing the organs. First, the bundles which constitute the organs are distinct, and no central point is perceived. The plurality of the organs, therefore, is as evident as the plurality of the faculties.” p. 267.

Now I do not perceive the justness of the conclusion, as set forth in this enthymeme. There is no central point perceived, but the bundles of the cerebral fibres are seen, and the deduction hastily enough adopted is, that the plurality of organs is as evident as the plurality of the faculties. But suppose the plurality of the faculties was denied, and that it was contended that the diversified modes of mental operation, which are termed faculties, were only *states* of the mind, what would become of the consequential proposition thus brought out of this enthymeme. But has Dr. Spurzheim, or any of the most enlightened advocates of the system, ever *demonstrated* these organs, as existing in the interior of the

cerebral mass? Has it been visibly and palpably shown where the lines of demarcation are to be seen circumscribing these organs, either in the more central portion of the brain, or on its periphery. If these separate and independent organs are not patent, is it not unphilosophic to contend for their existence?

I am aware that analogy, the ever  $\Delta\delta\epsilon\pi\alpha\tau\tilde{\omega}$ , with which, as by a lever, the disputant on either side of an argument, helps himself out of his difficulties, is appealed to very confidently, as being able to bring the system out of this dilemma. For if there is a homogeneity of texture seen in most of the organs, and a congruity of appearance manifested in various parts of the brain, what grounds have we for drawing the conclusion, that there is a difference of purpose subserved by each separate part, when all the parts differ from each other so little? It is contended that we cannot, *a priori*, determine the specific functions of any one organ of the body from its texture and appearance. To this, every one acquainted with the organization of the body, must yield a qualified assent.

The nicely arranged refracting structure of the eye would afford an exception, were we to suppose any one previously acquainted with the laws of light. The valvular apparatus of the veins, directed the mind of HARVEY to a discovery of the circulation, for he was led to believe, from the arrangement of the valves, that they did subserve that very purpose which his discovery demonstrated. But imagine that a person, totally unacquainted with anatomy, and destitute of any knowledge of the functions of the various organs of the economy, were to have shown him the curiously wrought mechanism of the inner ear, with the auditory nerves expanded on the cochlea, semicircular canals, and infundibulum, would he not, *a priori* and unhesitatingly say, that this organ was not designed for the same purpose as the eye, the organization being so essentially different: and so of the obvious difference observable in the stomach and liver. If all the organs were constructed alike, then the analogy might hold, but as the body is made up of that which every part supplies, and as each organ is so essentially diverse in appearance and

form, the illustration afforded by this resource is of an extremely tenuous character. But there is a higher kind of evidence adduced by the learned supporters of the system. It is the “Φιλόσοφια ἐκ παραδιεγμάτων,” the strong and pointed evidence gleaned from the field of observation. Dr. Gall was first actuated to the construction of this system, by observations made at an early age, on the skulls of his school-fellows. Now the question comes up, is the exterior part of the skull an exact counterpart of the brain? Three or four circumstances may influence the figure of the skull, independently of the natural form it takes from the brain. Artificial pressure may alter its shape very considerably, provided the pressure be applied in early life, when the bones are yet soft and flexible. Diseases of the bone may produce unnatural protuberances on the skull. The unequal distribution of the diploe may likewise vary the configuration externally in different skulls. The views of Dr. Spurzheim on this point are generally correct, and he has answered the objections urged against the system on this part of the discussion, with considerable acumen. When we raise the skull, however, to search for these organs, do we find them presenting their prominent heads on the periphery of the brain? Can there be pointed out to our searching glance any well marked designation of difference of function, by an obvious dissimilarity and incongruity of texture and appearance, in the various portions of the brain?

“The mind’s mintage may be characterized in the face,” but the mind’s mintage is hardly characterized on the brain. When carefully examined, the three lobes of the cerebrum appear similar in colour and texture. If the face exhibited such a sameness in its parts, though the brow, nose, and mouth, were left, the *mind’s mintage* would hardly be *charactered* in it. But perhaps to study organology, we must imitate Democritus; if so, then really “philosophy is harsh and crabbed as dull fools suppose.” But I am reminded “la moquerie est souvent une indigence d’esprit.”

To conclude this part of the discussion, let us see what

CUVIER, PINEL, TENON, PORTAL, and SABATIER, say in their report to the National Institute.

“It appears to us, 1st. That they, (Gall and Spurzheim,) have generalized rather in an inconsiderate manner, the resemblance of structure and of functions, in the various cineritious or ash-coloured masses which are met with in different parts of the nervous system.”

“2nd. That the idea which they entertain of a solution of continuity in the middle of the medullary matter of each circumvolution, which would permit its being unfolded like a pipe or a purse, requires to be expressed in more definite terms than they have hitherto done, so as to indicate that there is no complete proof of an absolute solution of continuity, but only of a more feeble cohesion. If even we were to adopt the greater number of the ideas of Drs. Gall and Spurzheim, *we would still be far from knowing the relations, uses, and the connexions of all the parts of the brain.*”

The zealous and assiduous manner in which Drs. Gall and Spurzheim have cultivated the difficult and complicated structure of the brain, and arrangements of the nervous system, deserves high praise. But their peculiar, and, if I may be permitted so harsh a word, “outré” doctrines of mind, I hold to be so utterly incompatible with the phenomena of the mind, and so discongruous with the sentient and indivisible unity of the human soul, that I must be allowed to combat their notions in a free and fearless, and at the same time candid manner. Perhaps there may be some obscurity thrown round this part of the discussion, from the difficulty which rises, when we attempt to render perfectly cognizable a subject which has never been fully and fairly developed. But avoiding the intangible and unprofitable speculation which has been so fruitlessly agitated in the *soi-disant* philosophy of past ages, as regards the inexplicable and mysterious connexion of soul and body, and the *ens* or essence of the soul, Drs. Gall and Spurzheim have taken up other and more approachable parts of the general subject of the mind, and entered upon the scrutiny of those parts with considerable

zeal and ability. In what local spot of its visible palladium the mind holds its sacred seat, they pretend not to determine. Nor do they speculate at all upon the materiality or immateriality of the mind, for they seem to be aware that these questions are the impassible Avernus over which the wings of genius will forever droop.

But they have speculated even in reference to the more accessible points, with a considerable latitude of remark. They have divided the mind into thirty-three faculties, and made each separate faculty dependent on a particular portion of the brain for its exercises and energies. Mr. Combe says, that "Gall and Spurzheim's philosophy may be summed up in two propositions: 1st. That the mind manifests a plurality of innate faculties, meaning by the word faculty a power or instrument of thought of a limited nature, having specific functions: and 2ndly. That each faculty manifests itself by means of distinct organs, and that these organs are different parts of the brain."\*

The position assumed is, that the mind manifests a plurality of innate faculties. I say assumed, for the position is not established by any consecutive series of argument, nor is a fair eduction of the supposed fact attempted. What precise signification are we to attach to the word faculty? If by faculty he merely meant a susceptibility the mind possesses of being impressed in a particular way, or a facility or aptitude it enjoys of acting on particular objects, then I agree with the learned authors of the system, that the mind does manifest a plurality of faculties—or to speak in more intelligible language, that the mind does possess a capacity, susceptibility, or power, from its very constitution, or original creation, to be acted on in a variety of ways, and to pass through an endless multiplicity of states or moods of thought. But if, agreeably to Mr. Combe, "a faculty be a power or *instrument* of thought of a *limited nature*, having *specific functions*," then I must contest the ground thus taken, for

\* Combe's *Phrenology*, p. lix. of Intro.

such a postulatum is scarcely to be upheld by the firm support of inductive evidence. What is mind? It is neither tangibly, visibly, nor audibly presented to our observation. It is not appreciable by any of our chemical tests. Has it any real existence, or is it "a mere nonentity, found only in the brains of dreaming philosophers?" Mind, like the principle of life, or the principle of gravitation, is only known by its effects. We all agree as to the phenomena of life and of gravitation, yet each man may construct a different theory to explain such phenomena. The mind is known by internal perception or consciousness to the individual who is the subject of its operations. It is known to the percipient intelligent beings around by the manifestations presented through the instrumentality of the external organs. Every rational mind, nay even the clouded intellect of the half reasoning imbecile, is conscious of the individual, indivisible unity and individuality of its sentient mind.

The memory is a general power or susceptibility of the mind variously modified, by which, not only impressions produced on the mind by external objects, but the fluctuating states of the mind itself, when suggestion after suggestion rises on its mirror in fleeting succession, are registered and kept. Would we contend that the memory is composed or made up of a number of distinct and independent faculties, because its general power is so variously modified? We should have a particular memory for every different feeling of the mind, as well as a particular memory for every system of facts and inferences, which have been treasured up by the accessions of a varied investigation.

Of the thirty-three faculties of amateness, philoprogenitiveness, inhabitiveness, adhesiveness, combativeness, destructiveness, constructiveness, covetiveness, secretiveness, self-esteem, love of approbation, cautiousness, benevolence, veneration, hope, ideality, conscientiousness, firmness, individuality, form, size, weight and momenta, colouring, locality, order, time, number, tune, language, comparison, casuality, wit, and of imitation, three or four are mere modifications of memory. Are we to dignify with the name of faculty each

modification of memory, or consider the mind endued with so many separate *instruments* of thought, because it exists in various states of activity? In perceiving, remembering, hoping, fearing, rejoicing, weeping, and the numberless other states or moods of mind, it is the same mind existing in a varied condition of thought and emotion. It is the same precipient and sentient mind, "tho' chang'd in all, yet in all the same."

What are these thirty-three faculties but the mind itself? If these faculties are instruments, then the mind is made up of nothing but instruments. What is a disease apart from its symptoms? If the increased action of the ventricles of the heart, the pain of the head, and the other symptoms of fever were absent, where would we look for the disease? I presume we would be in pursuit of the *universal a parte rei*, if we should search for a disease abstractedly from the symptoms, which alone denote its presence to our senses. So would we be searching for a mathematical point which has neither breadth, length, or thickness, which cannot be either felt or seen, but only imagined, if we were to try to find out what the mind is, destitute of its faculties. Suppose we were to say that the headache of a fever was only an instrument of the fever, or a deranged condition of the *primæ viæ* was merely a power by which the fever operated, would not we fall into a strange sort of logomachy? Strip matter of its divisibility, figure, hardness, and extension, and what is matter? Strip mind of memory, imagination, and the other states which we term faculties, and what is it? I cannot understand how a faculty, which is only a mode of mental operation, can be an instrument, and at the same time nothing but the mind itself. To say that a man's brilliant fancy was the individual instrument, or that the moral perception of a person was the power of the person, would be saying that the agent was the instrument, and the man the instrument of the man.

There is an argument, drawn from the fatigue the mind experiences in one continued train of labour, which is adduced by the advocates of the system. That our minds are liable

to collapse after strenuousness of effort, none will deny. But how is it that the simple recurrence of the same sound palls on the ear, or the repetition of similar views rise before the eye with no interest, though they once delighted us? The sense of taste soon becomes weary with the same food. The stomach loathes aliment of one kind long persevered in.

The love of variety is an ingenerate feeling of the mind. So the distaste we realize from the constant revolution of the same joys, to the boon of existence, and the load of mysterious ennui which bows us down, when the accustomed scenes around have no power to charm and interest.

The mind and nervous system are bound together in a temporary connexion, by a nexus or link, which I pretend not to understand. The diseases of the body do affect the mind, and the agonies or joys of the mind reciprocate an influence on the body.

We cannot long meditate on one subject without an impairment of the powers of the nervous system, which is reflected back on the mind. So we cannot employ our hands in one precise mode of manual exertion without fatigue, and yet by varying the action of the hand we can endure the exertion a longer period. Why is it that one tonic may lose its influence on the stomach entirely from repetition, and yet that organ be invigorated by the application of any remedy of the same character? Repetition blunts sensibility, and wears down the susceptibility of a part to be acted on by the specific impressions, whether of mind or remedial substances, or accustomed pleasures. This is a fundamental law of the system, and I pretend not to unravel the recondite nature of the nervous system any further. The mind has a power of voluntary action. If these supposed organs did exist, why should we not be conscious of employing them as instruments? We direct at pleasure our attention to the sensitive inlets of external impressions. We employ the eye to see at pleasure, and we are conscious of it. If the organs are played upon as so many strings of an instrument, then the mind should be conscious when it employs them. But is it the fact then any one can say that this part of the head is now

employed in remembering, and the other part the next moment is vibrating to the touches of the mind, in making the sweet music of fancy? We are conscious of using the brain collectively, for the purposes of thought, but not at all sensible of having distinct and particular portions of it operated on for particular processes of mind. The sovereign volitions of the mind direct the train of thought at pleasure into one or the other channel of reflection and meditation. If the organs lay such a strong hold upon the mind as to coerce its movements into the most dominant of them at pleasure, then phrenology

“Teaches us to mourn our nature, not to mend,  
“A sharp accuser, but a helpless friend!”

But if the mind has power to direct its energies in whatever manner it chooses, without respect to the organs, then they are mere *nervi alieni mobile lignum*; mere puppets moved by wires in the hands of another.

With regard to diseases of the mind, it is conceded by high authority, that “little has been found in the brains of maniacs to elucidate the disease, (mania,) that in numerous and accurate dissections of the insane no alteration from the healthy structure could be discovered.”\*

Cases of hydrocephalus are, I think, opposed to the doctrines under discussion. The manner Drs. G. and S. explain the unravelment of the fibrous structure of the brain, is not satisfactory to my mind. I will, however, merely relate two remarkable cases, and leave the reader to draw his own conclusions.

Dr. Delafield thus states the appearances on dissection, after giving a history of the case.

“On opening the head after death, the membranes of the brain were found gangrenous several inches around the puncture. They still contained about two quarts of a turbid fluid, which had the appearance of broken down brain mixed with water, and was excessively fetid. No part of the brain remained, except a small mass at the basis of the skull, which appeared

\* *Medico-Chirurg. Review*, for Sept. 1822, p. 283.

to be the pons varolii. The extremities of the nerves were seen floating on a pulpy semi-fluid matter, which covered the base of the cranium.

“Notwithstanding the great mass of the brain was completely destroyed, and its place supplied only by a volume of fluid, this child not only lived, but appeared to have possessed, during life, the full enjoyments of all its senses.”\*

The other case is given us by Dr. GLOVER. The dissection showed that “the brain was nearly all absorbed, but what remained of it rested with its pia mater on the base of the cranium. It had lost much of its consistence, indeed I found it impossible to distinguish the several parts of which it was composed, and on appealing to the gentlemen present, they, as well as myself, were of opinion that it was not larger than a hen’s egg; and yet, astonishing to relate, the child retained her senses to the last, and appeared to possess many of the faculties of the mind.”†

When an organ is found softened, the advocates of the system contend that the manifestation of mind dependent on this organ is weakened, and yet when all the organs are thus broken down in dropsy of the brain, they explain the integrity of the mind on the unfolding of the fibres.

I do not deny the intimate connexion existing between the mind and brain, for I have already enunciated that I am aware of this union. But it is the whole and not a part, a unity and not a separateness of function, which binds the mind in connection so inexplicable with the brain. Upon what peculiar state or condition of the nervous system depends the diversity of genius and disposition which we see manifested among men, I cannot tell. Why the hectic fever should be accompanied with the delusive visions of returning health, and hepatic affections so darken the mind’s horizon, it would puzzle even the advocates of the system to explain. Why it is at times that the mind operates with such felicity of thought, and rare combinations of images, we all

\* Am. Med. Record. vol. 4. No. 2. p. 451.

† Chapman’s Journal, vol. 2, p. 168.

must confess we cannot ascertain. Every man at times can say, "non eadem est mens"—and at another period without effort there will rise on the smooth and polished mirror of the mind, the gayest and most gorgeous imagery of fancy, and all the "gay disport of mimic life." There is a circling horizon which sweeps in its embrace the objects with which the mind of man may become acquainted, and beyond it the dim and indistinguishable shadows of conjecture are seen hovering.

Two objections might be urged against this system, which are derived from our belief of the spirituality of the mind. If the mind thinks only by the intervention of the particular organs, then our ideas are divisible, as the organs are divisible: and if the mind cannot act without the organs, then when the organs die the mind dies with them.\*

I cannot perceive how these conclusions are to be avoided. An organ is a multitude of particles, therefore an idea is composed of a multitude of separate parts.

The particles of the organs are removed, the ideas should be removed with them in the absorbing process of the vessels. The particles of the organs at death sink into corruption and the grave, the memory accompanies its organ, and so does the other faculties; and then what is the mind or soul without a recollection of the part or a sense of responsibility?

But what are the evidences adduced in favour of the system from observations made on the skull. It is ever to be kept in view in all our searches after truth, that accidental coincidence is not to be mistaken for inseparable and constant connexion. Two events may happen several times contemporaneously; but this simultaneous occurrence does not firmly establish the essential dependency of the one on the other event. The mind is prone to take up such concomitance for causation, for being averse to the vacillations of doubt, we too quickly rest down on erroneous conclusions.

\* Brown on the Mind, vol. 3.

The prevalence of an epidemic cough in Italy, during the age of astrologic art, at a particular conjunction of the planets, was proof sufficient to the hasty *generalizers* of that day, to induce them to believe in the influence of the stars in producing the disease. Hence the name of Influenza.

LAVATER captivated by the seductive blandishments of his physiognomic art, has shown us in his ponderous tomes, the liability the mind labours under to fall in with this sort of paralognism. Two kinds of proof are adduced in support of this system of physiognomy. The busts and skulls of the dead are brought forward to corroborate the views, and the skulls of the living are appealed to, to confirm the doctrine.

But how comes it that the prominences on BRUCE's or BURKE's head are so well known? Their peculiar faculties of mind are ascertained from history, and the map, perhaps, is thence constructed to suit the developments. From well attested facts I know the American craniologists have been frequently mistaken in trying to enucleate the predominant tendencies of the mind from the skull.

The following case which fell under my eye is sufficient to set aside many speculations, and will, perhaps, come across the path of at least a part of the entire circle.

Mr. H. a respectable citizen in this town, injured the upper part of his head in 1817; in a few months a beating tumour, supposed to be aneurism from anastomosis, was seen on the place injured, which was the vertex. After several operations for its extirpation had been performed without success, the bone being diseased, the dura mater participating in the disorder, and fungus after fungus shooting up, and after near three year's duration of the disease, there was a deep cavity of four inches in depth from the exterior part of the skull, and three inches in diameter, formed. The organ of firmness must inevitably have been swallowed up and lost in the cavity, and yet no man could have been more firm than himself to the very close of his protracted sufferings in death.

This very interesting case Dr. Johnston has promised to communicate to the medical public. From his constant at-

tendance on the case, he is able to give a minute narrative of its progress and ultimate destruction of so large a mass of brain. Mr. H. retained *all* his faculties to the last.

With regard to the different varieties of the human race, and the results of observation from comparative anatomy, I must refer to Dr. Warren's highly interesting work, "A comparative view of the Sensorial and Nervous Systems in Men and Animals." His conclusions are directly antithetical to those of Drs. Gall and Spurzheim on the developments found in the skulls of inferior animals.



ART. V. *Observations on Bilious Fever.* By GEORGE HOLCOMBE, M. D.

[Communicated by Dr. Emerson.]

THE fever which lately prevailed in the neighbourhood of Bordentown, was the ordinary bilious fever of the season, in rather a more aggravated and unmanageable form than common. In its worst cases it was strongly determined to the bowels and brain, producing acute inflammation in those organs. The only very unusual symptom which I have noticed this year, was a kind of tremor or paralysis of the tongue, preventing its protrusion beyond the mouth. This was an alarming symptom, denoting a malignant form of fever; so much so, that I can recollect the recovery of but one patient in whom it occurred. Deafness was a common symptom. I also remarked an uncommon degree of insensibility in many cases. Pressure on the abdomen gave little or no pain to patients, whose blood exhibited the highest marks of inflammation; the edges of its crassamentum being so introverted, (turned inwards,) as to give it a globular, or rather salt-cellar like appearance: a state of the blood, indicating acute inflammation, particularly, I have always observed, in the enteretic form.

*Treatment.*—The treatment which I pursued was free blood-letting, followed by calomel purges; which were per-

severed in daily, until salivation was induced, or the fever yielded. The usual adjuvants, such as emetics, diaphoretics, blisters, mercurial frictions, sponging, &c. &c. of course were not neglected. Bleeding and calomel, however, constituted the chief, and frequently the only remedies. Half-scruple doses were given every four hours, until three or four full evacuations were procured. The fever occasionally put on the form of a malignant intermittent, and assumed various, and sometimes very singular disguises. In such cases the sulphate of quinine, freely administered, proved infallible. How much does humanity owe to chemistry for this invaluable medicine! I can look back, in my practice, with regret, and recollect many fatal cases of malignant remittent and intermittent fever, which would have been promptly arrested and cured by the quinine. During this and the last season I have frequently prescribed it, during the exacerbation of remittent fever, (where delay was considered hazardous,) with the most gratifying results. The apprehension of cerebral and hepatic congestion, if the usual precautions be used, is utterly unfounded. I have used and seen it used in the most liberal manner, and cannot recollect a single case in which it has been obviously hurtful.

In the declining or sinking stage of bilious fever, I have prescribed this season large doses of calomel with remarkable success. As I believe the practice to be both novel and important, I will illustrate it by the following brief details of a case.

August 9th.—Miss —, aged sixteen, has been sick with bilious fever eleven days; has been bled freely, and well treated by an intelligent physician. Symptoms, head-ache, stupor, delirium, hot and dry skin; tongue dry, red, and glazed; pulse moderately full, and ranging between one hundred and ten and one hundred and fifty; diarrhœa urgent, of thin fœtid stools; great prostration of strength; excessive restlessness, &c. Let her take every four hours, twenty grains of calomel, until three or four consistent stools be procured. Drink and nourishment, barley-water. Mercurial frictions to the thighs and arms.

August 10th.—Has taken one hundred and twenty grains

of calomel, sixty before the first evacuation; passed several stools, the two or three last somewhat consistent. Stupor lessened; diarrhœa gone. The other symptoms stationary. Repeat the calomel and frictions. The head to be shaved, blistered, and drest with tartar emetic ointment.

August 11th.—The patient better. Stupor and delirium lessened, and all the symptoms meliorated. Has taken eighty grains of calomel, and passed four consistent stools. Continue the calomel and frictions, and dress the head with mercurial ointment.

August 12th.—Better; head very sore. Has taken forty grains of calomel, and passed several thin, and two consistent stools; pulse one hundred, softer and fuller; tongue reddening and softening; less stupor and delirium; continue the treatment.

August 14th.—Decisively better, in fact convalescent; stupor and delirium gone; pulse ninety, full and soft; gums and tongue red and moist, but neither mercurial secretion nor fœtor observable. Has taken during the last forty-eight hours one hundred grains of calomel, and passed several stools, most of which have been black, thick and copious. Says the consistent stools relieve her head, and thinks she feels stronger after each evacuation: and this is the remark of every patient, from whom calomel dislodges consistent stools. Left her in the care of the family physician, but have learned since that she recovered rapidly, without exhibiting, however, the slightest mercurial impression either in the breath or gums.

*Remarks.*—The above practice, bold as it may seem, has succeeded in curing some of the most desperate and unpromising cases of bilious fever I have ever seen, and which unquestionably would have sunk, or such at least is my opinion, under any other treatment. The triumphs which it has achieved have been among the most gratifying events of my professional life, and have added not a little to my confidence in the powers of medicine, in the sinking stage of malignant fevers. Large doses of calomel given in this form of the disease, instead of adding to the prostration of the pa-

tient, and increasing the diarrhœa, give strength and arrest the discharges; or, to speak more professionally, stimulate the liver into healthy secretion, and, in this manner, tranquillize the bowels. I have given one hundred and twenty grains of calomel in several cases, before a single stool was procured, and these too were cases in which the bowels, previously to the exhibition of the calomel were so relaxed, that a colliquative diarrhœa was rapidly exhausting the patient! Is this practice novel? Large doses of calomel, I am aware, much larger than I have found necessary to prescribe, have been given as evacuants in bilious fever, in the tropical regions of both hemispheres, and in the southern states of our own country, but never before, as far as I can learn, in the sinking stage of the disease, as alteratives; as medicines intended to supersede the use of opiates, tonics, and stimulants, the general, if not universal practice in that stage of fever, in which I have found calomel so signally successful.

But I wave the inquiry as to the origin of the practice. I merely wish to state its results in my hands. One of the most desiderated objects in medicine seems to me to be the establishment of correct rules for apportioning the doses of calomel to the different stages of fever. Two grains of this medicine will frequently act as irritants to the bowels, and increase diarrhœa, when twenty grain doses will check the serous discharges, and procure consistent stools. Again, twenty grain doses have sometimes proved irritants, when forty or sixty grains have been found to constitute the admissible dose. As a general rule, calomel, I think, may be administered fearlessly, as to quantity, and much benefit expected from it, whenever the patient labours under *delirium*, attended with a *dry and dark brown, or red and glazed tongue*; a *hot, dry and rigid skin, and diarrhœa*. It must be avoided, or cautiously administered, when the *tongue is moist and the skin and bowels relaxed*.

I have been sustained in my practice of prescribing calomel in full doses, in the sinking form of bilious fever, in consequence of having repeatedly observed its effects in the summer diarrhœa of children; a disease which strikingly resem-

bles the decline of fever. I have been in the practice, for the *last fourteen years*, of treating this disease, (cholera infantum,) almost exclusively by small doses of calomel, (after the manner of Ayre,) but have occasionally, whenever the exigencies of the case seemed to warrant it, administered large doses, from eight to twelve grains, and have generally been struck with the prompt and happy manner, in which the little sufferer has been relieved and cured. Upon the same principle, stimulating an atonous and exhausted liver, I have prescribed large doses of calomel in fever; and hence my doses vary from ten to sixty grains, being apportioned to the degrees of excitability which I may regard as existing in that great controlling organ of febrile diseases, the liver.

The usual method of treating bilious fever in its sinking stage, by means of opiates and stimulants, has always appeared to me a practice at best, of very questionable propriety: and I am disposed to conclude, after very ample opportunities for observation, that cordials and stimulants, except perhaps the occasional use of small doses of opium, are decisively injurious in every form and stage of bilious fever. This opinion may be incorrect, and perhaps extravagant, in the latitude which I have given it, but so far at least I am satisfied, that we frequently resort to diffusible stimulants, and thereby sacrifice patients which could be saved by awakening the energies of the liver by means of alterative medicines, particularly the calomel purges.

P. S. I perceive that I have omitted to advance, (which it was my intention to have done, and which I now do,) the following opinion:—That calomel, administered after the manner, and with the preparation which I have suggested, is as certain an antidote for bilious fever, as the quinine is for the intermitting form of the disease.

The profession is much indebted to Dr. Cooke of Winchester for his excellent essay: to Chisholm, however, after my own experience, I am indebted for the confidence which I entertain in the mercurial plan of treating bilious fever.

ART. VI. *A General View of the Hospitals of Paris.* By F. S. RATIER, M. D. Translated from the French by G. EMERSON, M. D.\*

THANKS to the care of a wise and benevolent administration, the hospitals of Paris, destined originally as retreats for the suffering and the unfortunate, have become the centre of a medical education, as pure as it is solid. From them clinical schools have branched out in all directions, under the superintendence of the most eminent physicians, whose instructions are gathered with avidity by studious youth. Each practitioner is seen taking advantage of his situation, to direct his efforts to some point on which by numerous researches and ingenious experiments, he may throw additional light, thus powerfully contributing to the progress and perfection of science and art.

Pathological anatomy, cultivated with equal zeal and success, reveals to physicians both the seat and causes of diseases, points out to them new methods of treatment, or at least, what must be regarded as a signal advantage, manifests to them those affections against which they ought to refrain from their curative attempts, and indicates where their efforts should be confined to a palliative treatment. Each year the administration publishes a medical collection, the fruit of the observations made in all the hospitals,† and it encourages by honourable rewards, those students who distinguish themselves by their labours and assiduity. It is by this means that the hospitals of Paris have produced so many men who now occupy a distinguished rank among physicians, of whom a great number sit, or might claim a seat, in the same rank with the professors in the different faculties of France. It was in issuing from the hospitals of Paris, that Professors DELPECH

\* This article forms part of an introduction to the author's second edition of his *Formulaire Pratique des Hospitaux Civils de Paris, etc.* Paris, 1825.

† This collection has appeared but once; but the causes which have prevented its publication, are, it is said, about to be removed, so that we shall soon see that fine enterprise continued.

and LALLEMAND have been appointed to deliver public instruction, by the Faculty of Medicine of Montpellier.

The numerous and useful modifications made in the interior arrangement of the hospitals, the new buildings erected upon the most approved plans, the efforts made to render the old ones more healthy by every possible method; the establishment in most of them of the proper apparatus for administering fumigations, baths, and shower-baths of every species; the public consultations by means of which a great number of patients are treated at their homes, all attest the active solicitude of the administration for the relief of human nature and the advancement of the healing art, whilst they place the hospitals of Paris in the first rank of institutions of this kind.

Notwithstanding all the hospitals present subjects of instruction, there are nevertheless some of them, which, on account of their extent, situation, and organized clinical schools, merit the particular attention of students and physicians from abroad.

### *Hôtel-Dieu.*

The Hôtel-Dieu, the most ancient and extensive of all the Paris hospitals, is likewise the most important, in consequence of the clinical instruction of which it forms the centre. This hospital, into which all kinds of diseases were formerly received, and thrown indiscriminately together, is now solely appropriated to acute diseases, both internal and external. The subjects of particular affections, such as the itch, tetters, syphilis and mental alienations, are directed to the several hospitals destined for their reception.

The surgical service, though confided to the care of Messrs. DUPUYTREN and MARJOLIN, is exclusively performed by the first of these celebrated professors. Daily visits repeated morning and evening, gratuitous consultations, surgical operations, are all accomplished by him alone, with a zeal and assiduity difficult to imagine. I cannot convey a better idea of the practice and opinions of this illustrious surgeon, than by subjoining the notes with which he has politely and kindly favoured me.

“The demolition of the houses in the neighbourhood of the Hôtel-Dieu, the access afforded to the air around and in the interior of the building; the closure of the wards considered as unhealthy, and the more wholesome condition of others from the reduction of the number of beds; the lowering of the casements even with the floors; the sending into other establishments, maniacs, child-bed women, contagious, cutaneous, or other diseases, ulcers, and the aged and infirm; the abundance and choice of all articles necessary for clothing and bedding, for the dressing of wounds, and for food and medicine; the order and regularity established throughout have made the Hôtel Dieu one of the most healthy hospitals of the capital. The sight and smell are there no longer disagreeably affected, as was formerly the case; scarcely any of those adynamic fevers are now observed, which were formerly produced in such great numbers by a confined air, charged with putrid miasmata. There are no more cases of hospital gangrene, and Mr. Dupuytren has been more than once obliged to close his course of clinical surgery, without having been able to exhibit a single case to his pupils. The operation of the trephine, performed in cases where its employment is indicated, and under circumstances which are not beyond all resource, has there succeeded like other operations, and only failed from analogous reasons.

“In retaliation, however, and probably in consequence of the pains taken to favour the access of air into the hospital, and renew and circulate it throughout the wards, we find there a great number of inflammations, as if, avoiding one extreme, is necessarily to fall into another. In effect, if we no longer see there putrid and malignant fevers, mortifications, and gangrenes, we observe pleurisies, pneumonies, and peritonitis. These are the causes of death in the greatest number of persons who die in the surgical wards. All the bodies of those who have died within these wards for the last six years, have been opened without exception, and this practice has established an assertion often repeated by DESAULT, without his being able to furnish the proof, that the greater number of those who die during the treatment of surgical affections,

are destroyed by internal inflammations, which are most often to the number of two, three, or four in the same subject."

"This observation has not been lost in the treatment of surgical diseases. Diluents and refrigerants have in most cases taken the place of tonics; blood-letting and leeches, been substituted for excitants and stimulants. Hardly has there been employed one pound of bark internally, and a few pounds externally, upon nearly three thousand patients, admitted and treated each year in the surgical wards."

"Fractures are there almost always treated by position: some of them, such as those of the neck of the femur and the neck of the humerus, by this method alone. The others by position aided by contrivances, rather adapted to prevent motion than to act with force upon the members. Permanent extension is there never used in any case. The frightful apparatus for the reduction of fractures and luxations has been banished from it, and has given place to milder methods. Hernias are operated upon immediately on the admission of the patients. Operations for cataract are performed by couching, and at the bed-side of the patients, which prevents the evils resulting from their removal after the operation."

"The mortality is there reduced, in ordinary years, to one in eighteen, nineteen, and twenty. The operation for stone succeeds in five out of six cases. The operation for hernia upon three-fifths; that for cataract upon seven-eighths; that for fistula lacrymalis, by the introduction of the gold or platina canula, upon nineteen-twentieths."

Independently of the surgical clinic, there is likewise at the Hôtel-Dieu a clinic for medical instruction, which the physicians of the establishment take charge of in rotation, every three months. These are Messrs. RECAMIER,\* PETIT, HUSSON, ASSELIN, BORIE, DE MONTAGU, and GEOFFROY.

Mr. Récamier, one of the most eminent physicians of the

\* Since the new organization of the Faculty, Mr. Récamier is professor of the clinic of the Ecole de Médecine, and no longer of that founded by the administration.

capital, and stimulated with the most ardent zeal for the art which he practices with so much celebrity, has distinguished himself by the sagacity with which he recognises diseases and their various complications, by the certainty of his prognosis, and the skill with which he employs the infinitely various means of which he avails himself in his practice. In the midst of the medical systems, which succeed each other with such rapidity, he gives to no one absolute preference, borrowing from each what appears advantageous. Passing alternately from the expectative plan to the most energetic medical practice, he leaves those who wish to form for themselves an exact idea of his doctrine in the greatest embarrassment. Like all the other hospital physicians, he applies himself to researches in pathological anatomy and the *Materia Medica*. He has contributed in no small degree to the progress of science and the art.

It is much more easy to make a statement of the opinions of the other physicians of the Hôtel-Dieu, who are divided, although in an unequal manner, between the ancient medical doctrine and that which appears to be about to succeed it. On the one side, Messrs. de Montagu, Petit, Borie, and Geoffroy, appear to have preserved in their practice, the ideas of the ancient humoralists, and the opinions of BROWN upon the employment of tonics and stimulants, supporting themselves by favourable results and by theories more or less seducing. On the other side Mr. Husson, adopting the principles of the chief of physiological medicine, seems, indeed, to carry his opinions still further. Prescribing, in almost all cases, diet, mild drinks, local and general bleedings, baths, and relaxing applications, he embraces in one complete proscription the greater part of the usual therapeutic means, and his general success has confirmed him in the practice to which he has for several years considered it his duty to confine himself.\*

Although he speaks in less positive terms upon gene-

\* Such was the practice of Dr. Husson when this was first published, since which new reflections and experience have led him to adopt the contro-stimulant doctrine.

ral or particular modes of practice, Mr. Asselin, has nevertheless, distinguished himself by the prudent deliberation which he observes in the treatment of diseases. Persuaded that nature often effects the cure, when her course is not impeded, he directs all his efforts towards the removal of those causes which predispose to, or maintain the morbid condition, by a well directed regimen, and especially by abstaining from the employment of violent measures.

### *Hôpital de la Pitié.*

This building is a species of relief-ward to the Hôtel-Dieu and the Hôpital des Vénériens, from the first of which convalescents are sent, together with subjects affected with chronic diseases, there to await their destination in some particular hospital. One of its divisions is appropriated to the reception of girls of the town affected with syphilis. These are placed there by the police, and treated by the physicians and surgeons of the Venereal Hospital. This service is altogether distinct from that of the rest of the establishment, of which Professor BECLARD is the chief surgeon. The physicians are, Dr. SERRES, known by his curious labours upon different subjects of general and pathological anatomy and physiology, and Dr. BALLY, who has devoted himself to researches upon various parts of the Materia Medica and therapeutics.

### *Hôpital de la Charité.*

This hospital, considerably enlarged and newly furnished with the necessary apparatus for the administration of baths and fumigations of all kinds, is regarded as the second hospital of Paris. The duties of the surgical wards are confided to the attention of Professors BOYER and Roux, entrusted by the Faculty of Medicine, one with the delivery of lectures upon clinical surgery, the other with instructions upon medical practice. The physicians of this establishment are Messrs. FOUQUIER and LERMINIER; M. CHOMEL is charged, independently of the service of the wards, with gratuitous consultations.

Mr. Fouquier, since the newly organized faculty has placed

him in the chair of internal pathology, no longer delivers clinical lectures, properly so called, but continues to expound at the bed-side of each patient the causes and symptoms of diseases, to show the prognostics and diagnostics, and in fine to point out the indications and basis of the treatment.

This professor, whose course is assiduously followed by a great number of medical students, displays in his practice that prudent procrastination which leaves something to be done by the preserving efforts of nature, and that prompt and energetic decision which changes or modifies a vicious tendency, and snatches the patient from certain death. An attentive and scrupulous observer, uniting a rare degree of freedom with a penetrating glance, he has distinguished himself by the precision of his diagnosis and the certainty of his prognosis, which he never pronounces until after mature examination. Far from seeking to dazzle by that pretended perspicacity which professes to discover the nature of diseases by the first glance, he on the contrary, instructs his pupils to carry to the investigation of diseases that minute attention and prudent delay which leads to positive results, or at least makes us avoid those errors which expose the dignity of the art. His practice is simple and rational, and if he sometimes takes the liberty of experimenting, it is with that prudence and reserve imposed by a sense of honour and the high responsibility which rests upon the head of the physician.

An absolute stranger to the spirit of systematising, and following the path traced by observation and experience, he has long since pointed out in his theoretical and practical courses, the frequency of inflammations and the necessity of insisting upon the antiphlogistic treatment even when they have assumed a chronic form. He has also taught that genuine fevers were very often to be regarded as symptomatic of obscure inflammation; but he nevertheless supposes that there do exist genuine fevers, that is to say, where the excitement is general, and shows itself in no particular part, so as to be regarded as the local cause of the phenomena. This professor has for a long time contended against the abuse of stimulants and tonics in the treatment of acute diseases; and yet

he cannot believe that the gangrenous inflammation of the intestines and skin, which occurs in adynamic fevers and carbuncle, are of the same nature and to be cured by the same means as the inflammation of those parts which constitute dysentery and erysipelas.

In many diseases Mr. Fouquier pursues a peculiar practice. In the painters' colic for instance, he has believed it his duty to leave the beaten track, and has added to the treatment, the efficacy of which has been confirmed by long experience, those modifications which a rational mode of treatment demands. Acute rheumatism is regarded by him as an inflammation, and treated accordingly: but he prefers the application of leeches and cataplasms, aided by gently diaphoretic drinks and warm baths to venous bleedings, which often induce a protracted convalescence. This plan has most frequently succeeded, with him, against those chronic articular rheumatisms, which have produced a species of ankylosis. In affections of the nerves, he generally first commences with blood-letting, either venous or capillary, which he follows with blisters not applied over the course of the diseased nerve, as CONTUGNO would have him do, but on the opposite side of the member.

The number of nervous diseases which appear so great to those who observe superficially, is greatly diminished by the practitioner who exerts himself to refer each train of symptoms to the lesion of some organ. Yet notwithstanding this reduction, the affections which we are compelled to denominate nervous are very numerous. Mr. Fouquier admits the existence of diseases purely nervous, that is to say, in which our present means of investigation have not yet been able to discover any material lesion to which they can be referred. He holds this opinion relative to the asthma, which Mr. ROSTAN, of the Salpêtrière, regards as dependent upon aneurism of the heart.

It is in the treatment of nervous diseases, such as epilepsy, hysteria, and hypochondriasis, that he has made trial of the extracts of poisonous plants, as the henbane, belladonna, poisonous lettuce, &c. together with other substances but little

known, as the distilled water of the cherry-bay and the hydro-cyanic acid.

Dropsies have furnished a particular object of study for Mr. Fouquier, and he has made upon this subject many researches relative to the operation of diuretics, of which we shall merely observe, en passant, and without detracting from the information which he will doubtless some day publish, he has carried the doses higher than most other practitioners. Of late, after the experience of Dr. Ségalas, he has made trial of the *urea*, in which he has discovered a very energetic operation upon the urinary organs.

This physician holds no particular opinion relative to the venereal disease. He usually employs against that affection, after having subdued the local symptoms by the ordinary means, pills of the Neapolitan ointment,\* from which he has left out the soap which enters into the composition of the pills called *Sédillots'*: he likewise administers the liquor of *Van Swieten*, and thinks with Mr. CULLERIER, that this medicine does not merit the reproaches which have been bestowed upon it, and above all, that it is incapable of producing phthisis pulmonalis, although it may sometimes hasten the development of that disease in subjects predisposed to it.

The original predisposition to tuberculous or cancerous affections, has been the subject of frequent controversies. Mr. Fouquier admits, that during most of the time the predisposition exists, although it is possible to escape it, and that the diseases which it ordinarily produces may manifest themselves in an accidental manner.

In aneurisms of the heart and large vessels, his treatment consists of local and general bleedings: most frequently he has recourse to the latter to empty the vascular system. He repeats them according to the facility with which the blood is again formed, continuing their employment till the termi-

\* The *Neapolitan ointment*, consists of one part of mercury rubbed down with six of fresh lard. Two drachms of this mass, with q. s. of powdered liquorice is divided into one hundred and forty pills. The dose may be increased to six a day, and about two hundred are generally sufficient for the course.—[*Translator's note.*]

nation of the disease; and has often observed venous blood-letting, to use the expression, revive patients who were almost suffocated. Far from favouring the increase of symptomatic anasarca, these evacuations seem on the contrary to facilitate the absorption of the effused fluids, and to second the action of diuretics. When this resolution has been long waited for, and the distension of the skin is considerable, Mr. Fouquier is in the practice of making punctures with a very sharp-pointed lancet, sufficiently deep to reach the cells of the cellular tissue, which produces a rapid diminution of the swelling. Long experience has shown this practitioner that there is nothing to be apprehended from gangrene, as some authors have stated; but it is indispensably necessary to pass entirely through the skin, instead of confining oneself to a mere division of the epidermis by drawing over it the edge of a lancet. To these means there is added a spare diet with some diuretics to arrest the progress of effusion, some preparation of squills and digitalis, which are considered as diuretics, and as diminishing the force of the contractions of the heart by acting upon its sensibility.

Mr. Fouquier has published but a very small number of memoirs. He has, in the mean time, made known to those persons who have assisted at his clinical lectures, the results of his labours upon divers points of medical practice, and of his researches upon various medicinal substances, upon the nux vomica, the acetate of lead, the extracts of henbane, belladonna, hemlock, stramonium, poisonous lettuce, and the rhus radicans; upon the distilled water of the cherry-bay, the prussic acid, of which some are already published, whilst others will soon be presented to the Royal Academy of Medicine.

It is a subject of regret that the clinical lectures and extensive patronage of Mr. Fouquier prevent him from devoting himself more to the labours of the closet. But does he not amply pay his debt to society by the well-instructed pupils which he every day forms?

The medical service is in part performed by Dr. Lermnier, a distinguished practitioner, who has had the kindness to communicate to me some notes, embracing a general view

of the most conspicuous points of his medical opinions. "I treat continued fevers by simple diluents, as long as no precise indication remains to be fulfilled. If an assemblage of inflammatory symptoms exists, or a state of plethora, I prescribe general bleeding. If sanguineous congestions threaten any parts, I divert them by the application of leeches. It is these various congestions which fix my particular attention in the course of fevers. I have, for instance, often applied leeches with advantage to the neck or behind the ears, even at a very advanced period of low fevers, whilst the adynamic stage was already formed.

"In all cases where a profuse diarrhœa exists, I moderate or arrest it by the application of leeches to the anus. We have often observed the stupor disappear and the forces revive after the employment of these means. But during the existence of genuine adynamia, I resort to tonics, and the preparation I generally employ is the aqueous infusion of bark. I apply revulsions, &c. at the same time.

"When I observe that assemblage of symptoms generally designated bilious, I do not hesitate to employ vomits. They are particularly useful in abridging the course of the disease. In intermittent fevers, my chief dependence is in the sulphate of quinine, except in some cases where it has appeared to give rise to nervous affections.

"I attack acute rheumatisms by general bleedings, which I repeat very often, and until the cupping has completely disappeared, or has become less distinct. Leeches in rheumatism afford great relief to the pain in the parts to which they are applied, but in the mean time it is apt to remove to other places. This does not take place after venesection.

"I have often expedited the resolution of certain pneumonies, which have a tendency to assume a chronic form, by substituting mild tonics for simple lenitives, especially the decoction of polygala and kermenised juleps.

"In those colics occasioned by metallic preparations, I follow throughout the old treatment of the *Pères de la Charité*, and I have even employed it successfully in cases where there existed a strongly marked febrile excitement.

The practice of Mr. CHOMEL, resembles very much that of Mr. Fouquier, except that he has shown himself a more zealous partizan of the doctrine of fevers, and of the employment of tonics and stimulants. This physician has produced facts, and even cures in support of his method, upon which alone enlightened and impartial physicians should rest their decision.

*Hospice de la Clinique Interne.*

This Hospital is appropriated to the instruction of clinical medicine, under the superintendence of the Faculty of Medicine. Two professors, Messrs. LAENNEC and CAYOL, are alternately charged with its duties, but a new arrangement is shortly to be made, by which each will have assigned to him separate wards and lead the two clinical courses.

Mr. Laennec, the name of whom is associated with so many splendid researches in pathological anatomy, has still further increased his reputation, by giving physicians a new mode of exploring the organs contained in the cavity of the chest, and of thus perfecting the diagnosis and the treatment of its diseases. His medical practice is bold, and often differs from the beaten tracks.

Mr. Cayol seldom permits himself to substitute an empirical practice for a rational course of treatment, unless it be in desperate cases. He prefers, in general, the expectative plan, and does not act, except from pressing indications, relying greatly upon the efforts of nature. He admits that a very great number of fevers should be considered as symptomatic of local lesion, but nevertheless acknowledges that genuine fevers do exist, that is to say, commotions of the system, to a greater or less degree, which have not been preceded by any organic lesion. This is the doctrine which he professes, and which he teaches to the pupils of his clinic, together with remarks upon various other subjects. As to the treatment of continued fevers, when they present indications of gastric and intestinal irritation, Mr. Cayol first examines whether this irritation precedes the fever, or whether it is the result of direct excitement produced by the indiges-

tion of stimulating remedies. It is but seldom in these last mentioned cases, that he has recourse to leeches to the anus, or to the epigastrium; but with the exception of him, they are very frequently thus employed in the hospitals. When, on the contrary, the patients have been dieted and subjected to a moderate temperature from the first attack of fever, Cayol observes and shows, that if they are still in the turgescient stage, the redness of the tongue is always in proportion to the heat of the skin; but that this redness, accompanied with thirst, and sometimes with a little tenderness of the abdomen, is only connected with a state of congestion: it is found to diminish at the same time with the heat of the skin without any recourse to leeches, and during the existence of the general symptoms of plethora, Cayol prefers general bleeding to that from the capillaries.

Inflammation of the mucous membrane of the respiratory passages is often complicated with genuine fevers, and is in general more dangerous than inflammation of the alimentary canal. It often supervenes in the course of a continued fever of a low type, and remains unperceived, without a careful examination by means of the cylinder. Cayol combats it with almost constant success, by means of scarified issues applied over the sides of the breast towards the attachment of the denticulatus. This method is also sometimes employed to subdue simple sanguineous congestion of the lungs in continued fevers.

Continued fevers of the general constitution, usually commence with a state of sanguineous turgescence, which calls for diluent drinks, and sometimes for bleeding. As regards the local affections which accompany them, if they have not preceded the fever, and if they do not present the characteristics of inflammation, but only those of congestion, Cayol is little concerned about them, abstaining from the employment of local bleedings, which, by impoverishing the capillary system retard the efforts of the crisis, and prolong indefinitely the period of convalescence. When the antiphlogistic indications are exhausted, the head alone remaining a little disturbed, he applies blisters to the legs, and it is not until seve-

ral days after, and when the symptoms of cerebral congestion do not yield, that he resorts to blisters to the back of the neck.

In that second period of fever, whilst there still exists the mucous rattle in the breast, Cayol employs with success the decoction of *polygala senegæ*. At a later period if petechia or sloughing over the sacrum occur, or if the blisters present some symptoms of mortification, (profuse suppuration, ichorous or sanious, with acute pains,) the use of bark is certainly indicated. (Commonly one drachm of the soft extract in a mucilaginous mixture, and sometimes the acidulated decoction of bark.) In the mean time, Cayol washes the blister several times a day with a strong decoction of bark, and anoints it with opium cerate. Such is the practice pursued in continued fevers by this practitioner, who loses very few patients.

Although the medical constitution has within the last twelve or fifteen years been generally inflammatory, some bilious fevers are yet to be met with; the last three months of the clinic has presented eight or ten cases. Whenever Cayol observes at the commencement of a fever the union of these three symptoms, a coated, moist and yellow tongue, a yellow tinge and frontal or orbital cephalalgia, with a general state of languor, he commences with an emetic, and always with success: he then employs lemonade, acidulous drinks, and apozems of chicory. The evacuations are then always easy, and followed by prompt relief. If these bilious symptoms are accompanied with some indications of an inflammatory action of the stomach or intestines, such as redness and dryness of the edges of the tongue, considerable tenderness of the stomach and abdomen, Cayol precedes the emetic with a bleeding. When the bilious symptoms only manifest themselves in the second stage of the fever, he prefers purgatives to vomits. The constant observation of this professor has shown, that on all occasions, when these bilious symptoms have not been combated with mucilaginous drinks and leeches, the fever has been indefinitely prolonged, and after it has terminated, the patients have long remained

in a state of languor and uneasiness, which often ends in farunculés or other eruptions.

Mr. Cayol rarely employs either general or local blood-letting in the first stage of eruptive fevers, at least where there is no very violent local inflammation. But after the example of SYDENHAM, he very often bleeds from the arm during the convalescence of these fevers, particularly in small-pox and scarlatina. This practice has always succeeded with him, and for a long time during which he has used it, he has much more rarely seen those obstinate inflammations, the grievous relics of eruptive fevers. He does not purge during the convalescence of these fevers unless there are very particular indications.

In the treatment of acute inflammations in general, he repeats the venous blood-letting until the fever subsides. When the fever has diminished, he resorts to local blood-lettings, and when it has ceased, he bleeds no more, (except in cases of relapse,) and then begins with derivatives, at first applied at a distance, and afterwards nearer to the seat of disease.

In acute peritonitis, after a sufficient quantity of blood has been taken, two large blisters to the internal parts of the thighs have appeared to Mr. Cayol exceedingly efficacious in abridging the disease and preventing suppuration.

To favour the resolution of chronic inflammations, (hepatitis, pleuritis, pneumonia, peritonitis, &c.) and to start certain organic degenerations, such as phthisis pulmonalis whilst it is circumscribed, scirrhus of the stomach, renal phthisis, various degenerations of the uterus and bladder, Mr. Cayol has often employed with unlooked for success, the seton formed with two scarifications, which he generally makes with caustic potass, and sometimes with the moxa, through which he afterwards passes a flat or roundish wick, accordingly as he wishes to produce more or less suppuration. This species of issue is the one which has appeared to him the most energetic. During its action he often contends advantageously with the hectic fever, (especially when it has regular exacerbations with chills,) by means of the bark, in the form of extract alone or combined with a few drops of laudanum.

He has thus succeeded in arresting the hectic fever, and in prolonging for a great while the duration of cases of incurable consumption.

*Hôpital Saint-Louis.*

Of all the hospitals destined to special objects, that of Saint Louis is undoubtedly one of the most important. In it are placed the subjects of cutaneous diseases, such as tetters, the itch, tinea, and a great many scrofulous, scorbutic, and cancerous affections. Independently of those patients received and treated within the hospital, a public consultation is open to all those whose affections are so slight as not to render it necessary to quit their ordinary occupations whilst under treatment. To these, cards are delivered, with which they come daily to take the baths or fumigations suited to the nature of their diseases. This establishment is confided to the care of Messrs. RICHERAND, professor of the Faculty of Medicine, and JULES CLOQUET for the surgical department, and to Professors ALIBERT, BIETT, MANRY, and LUGOL for the medical service.

Mr. Alibert, whose numerous engagements have not prevented him from devoting himself to the instruction of young physicians, delivers annually a clinical course upon diseases of the skin, during which he exhibits to his audience the most remarkable examples of those affections, of which he has given such frightful but faithful pictures in his magnificent work. He has tried against these grievous disorders different remedies with more or less success. In the work alluded to, there may be found those preparations which he has found most frequently successful. Charged alternately with the wards appropriated to the treatment of psora, and enjoying the inappreciable advantage of repeating and comparing experiments upon a vast number of subjects, the physicians of the Saint Louis hospital have fulfilled the general expectation, by devoting themselves to researches relative to the nature of this disease, and the numerous modes of treatment successively employed against it. Doctor Manry has confined himself to an investigation of the means which ought to be pre-

ferred, as relates to the expense, injury to the linen, smell, and circumstances both local and general. He has made trial of twenty-two preparations, each upon an equal number of patients, and summing up the duration of the treatment in all the cases, he has ascertained the mean term required by each method. It is from this labour, executed under the inspection of Mr. Manry, with equal precision and ability by Doctor Mélier, at that time an internal student of the Saint Louis hospital, that I have extracted various formulas and the notes which refer to them.

I have also availed myself of a more extensive work published by Mr. MOURONVAL, entitled, *Researches and Observations on Psora made in the Saint Louis Hospital at the Clinic of Mr. Lugol, during the years 1819, 1820, and 1821*, in which the experiments appear on a much larger scale, whether we take into consideration the number of cases or the preparations employed. Without confining myself to that part of the work exclusively dedicated to the history of psora, and in which, by the way, the author positively denies the existence of the *acarus scabiei*, of the *sarcopte* which have succeeded it, and of the whole tribe of psorous animalculi, he has definitively proved by numerous and well made experiments, that the itch can be cured with greater or less facility, by stimulating applications to the skin in the form of baths, fumigations, lotions, frictions, &c.; that it will even yield to aqueous vapour baths, and that the internal treatment to which formerly so much importance was attached, is useless except in cases of long standing, where the extent and violence of the disease have rendered it so habitual to the constitution, that it would be imprudent to remove it without proper precautions.

Doctor Biett, specially charged with the wards appropriated to the treatment of the different species of tetter, has made numerous interesting experiments upon the various substances employed for the cure of this class of diseases, and devoted himself to curious researches relative to epilepsy and some other affections, the obstinacy of which has often baffled the resources of medicine.

*Hôpital des Vénériens.*

This hospital, which is exclusively dedicated to the treatment of syphilis and the affections depending upon it, is confided to the care of Messrs. Cullerier, the uncle and nephew, and to Professor Bertin. It is here that every form and variety of this disease is displayed, and that the effects of the curative means employed against it can be well examined. From the numerous researches made by the practitioners who preside over this establishment, nearly the whole of the following doctrines result, although the developments belong to another work.

Notwithstanding the different forms assumed by the venereal disease, the basis of the treatment is and ought to be considered always the same. It is only to be modified according as the affection is primitive or secondary. The local treatment is regulated according to the symptoms, and their various degrees of intensity.

Mercury is regarded as a specific against the venereal disease, and those cases in which it has failed are exceptions which ought not to be considered as invalidating the general rule. For a long time it has been regarded as the only specific, but experiments made in Spain and Italy have shown that vegetable substances, and especially sudorifics, assisted by a warm climate, are able to overcome the disease without the use of mercury.

The tisan of FELTZ ought not to be viewed as a purely vegetable drink, when it is considered, that besides the various plants which enter into its composition there are added the isinglass of fish and sulphuret of antimony, which last always contains more or less of the black oxide of arsenic. Some facts, and especially the trials made in the Saint Louis Hospital, in different cases of venereal eruptions, appear to prove that this oxide is the most predominant property in the tisan of Feltz.

In contending with the primitive symptoms, the liquor of Van Swieten, in doses of half a grain per day, a simple tisan of barley, and often even no tisan, constitutes the most common treatment.

If it happens that patients labour under troublesome catarrhal affections of the breast, or exhibit symptoms of incipient phthisis pulmonalis; if the abdomen is the seat of irritation, or if the liquor occasions pain in the stomach or vomiting, Mr. Cullerier puts his patients under the use of frictions made with mercurial ointment, or administers to them what are called Sédillot's pills, (made of the Neapolitan ointment and soap,) or the mercurial pills of Hahnemann, (composed of the black oxide of mercury.) These pills have the inconvenience of acting upon the mouth with the same rapidity as the frictions; this, however, does not prevent their exhibition in cases of primary syphilis. Mr. Cullerier never employs them in secondary affections.

If a patient presents himself with secondary symptoms, the great reliance which Mr. Cullerier places in the liquor induces him to administer it equally, and it may be said with such remarkable success, that if some cases should resist and continue to grow worse, it might be supposed owing to the negligence of the patient in the course of his treatment. Frictions also appear to Mr. Cullerier an energetic resource against constitutional syphilis, and he is almost as partial to their employment as to that of the liquor of Van Swieten. He gives them the preference with subjects who do not bear this last preparation well, and he obtains the most advantageous results from their use.

When he administers the liquor in inveterate venereal affections, he usually associates with it the sudorific tisan alone or sweetened with the sudorific syrup; but when the patients appear feeble, he mixes this syrup with that of cinchona or the *antiscorbutic syrup*.

The tisan of Feltz is employed with a success truly surprising, and Mr. Cullerier is in possession of extremely interesting facts upon this subject. He administers the tisan when the secondary venereal symptoms, such as exostosis, fixed pains in the bones, periostosis, serpiginous pustules, ulcerations of the soft parts, caries of the bones, and of the cartilages of the nose and mouth, after having yielded several times to the treatment by the liquor or by frictions, reap-

pear. This method almost constantly succeeds, and even in a very inconsiderable time, at least when the disease has not produced too deep ravages. Mr. Cullerier, the nephew, has tried the *Tisan of Arnaud* in the Maison de Santé des Vénériens, but has not made known the results of his experiments. Mr. Cullerier, the uncle, thinks that it is similar to the *Tisan of Feltz*.

The muriates, (hydro-chlorates,) of gold and of platina, have not realized in the hands of this physician the pompous recommendations of their inventors, and their use has accordingly been discontinued.

He considers baths containing a solution of the deuto-chloride of mercury as equally active in curing the venereal disease, but he rarely employs them in his hospital, because this mode of administering the sublimate requires so many precautions.

The local means are very limited. They consist of simple or mercurial cerate upon the ulcerations, most frequently a little lint wet with a purely emollient or narcotic decoction, cauterization with powdered nitrate of silver, and anointing them with the *onguent égyptiac* when the granulations are fungous, and with the digestive ointment when they are indolent.

As regards excrescences, their excision is not to be attempted until the specific treatment is terminated or far advanced. This mode is most commonly successful: nevertheless there are some patients with whom these vegetations seem to spring up with greater force, and in such cases recourse is to be had to cauterization with the nitrate of silver in powder, the hydro-chlorate of antimony, and the nitrate of mercury. Notwithstanding the activity of these applications, excrescences are often met with which resist them.

Rhagades of the anus are likewise treated by caustics. The actual cautery is the kind which has succeeded best with Mr. Cullerier. Those found on the hands and feet call for great attention to cleanliness, with rest, and may always be cured with the liquor or the frictions, together with the local application of simple or mercurial cerate.

Venereal pustules do not require any particular treatment. For the mucous pustules of the anus, the thighs, the vulva, or the scrotum, Mr. Cullerier recommends great cleanliness and gentle frictions upon the affected parts with the mercurial cerate. He considers this plan as highly advantageous.

In exostosis, periostosis, &c. leeches have never appeared of much use, and the general treatment is that which has been found most efficacious.

Blennorrhagia is a symptom of syphilis, relative to which there is much diversity of opinion. Long experience has convinced Mr. Cullerier, that it is very often followed by a general infection, and he also thinks that this morbid phenomenon ought to be combated by the means adapted to the treatment of constitutional syphilis.

[To be continued.]

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ART. VII. *On Miasm as an alleged Cause of Fevers.* By  
JOHN BELL, M. D.

AS in the following essay I shall be led to form inferences, which are at variance with the creed of the medical profession at large, it is but just to them that I explain in advance the reasons which induce me to hold the opinions therein given, and thus exculpate myself from any charge of overweening desire at originality, by substituting, as is too often the case, paradox for patient investigation, and scholastic syllogisms for fair induction. I would fain steer clear of the two extremes pointed out by BACON. “*Duplex autem est excessus: Alter eorum, qui facile pronunciant, et scientias reddunt positivas et magistras; Alter eorum, qui Acetalepsiam introduxerunt, et inquisitionem vagam sine termino.*” [Nov. Organ.] While I protest against the *facile pronunciare*, I by no means wish in this or any other question to encourage the *inquisitio vaga*.

In preparing materials for the course of lectures on the In-

stitutes of Medicine, which it is my province to deliver every summer, at "The *Medical Institute of Philadelphia*," I was necessarily induced to enter largely into the subject of ætiology, and especially of that branch of it derived from the knowledge of medical geography. To this study I was sufficiently prone, as well from my conviction of its utility, as from circumstances having at different times placed me in great varieties of climate both on sea and land. But it was not enough to echo the general and too seducing views of such writers as MONTESQUIEU and CABANIS, on the influence of climate as modifying national character, and tending to the formation of national laws. My object was to learn the precise effects of localities on the human frame, and to know how it was impressed by elevations of soil, exposure to particular winds, vicissitudes of temperature, moisture, and dryness. In this examination, if I except the treatise of HIPPOCRATES *de aquis aëre et locis*, I was without any sure guide. To embody then the notices of this nature scattered through travels and medical writers became my first task and duty. I commenced with the effects of the circumfusa, or all the evident and appreciable properties of the atmosphere; first considering them in their more simple state, applied alone and with intensity, as heat, cold, winds; and next observing their rapid succession and alternation, as in the several seasons, particularly spring and autumn.

Thus furnished with a series of valuable records and practical deductions, I next ventured on the subject of endemic diseases, in which I was of course required to investigate the reputed origin and mode of transmission of marsh miasmata or effluvia, or, as it is now more fashionably called, malaria. LIND, CLARK, JACKSON, BANCROFT, and JOHNSON, supplied me with copious details on this part of my course; and on their authority I ventured to present a summary of the generally accredited opinions upon this subject. I could not avoid, however, in the progress of inquiry, being forcibly impressed with the number of causes which modified, or in a great measure neutralized the power of miasm in the production of febrile diseases. The incipient doubts,

which now began to stagger my belief in the miasmatic doctrine, were by no means lessened when my views were extended to the study of epidemics. I found that the same fevers which are usually treated of as endemial, do not unfrequently become epidemical; and that this conversion or greater extension of such diseases was not pretended to be explained by an extrication of more miasm than usual, but by great irregularities in the seasons and abnormal vicissitudes in the weather. The perusal of GIANNINI on Fevers, an analytical notice of which is to be found in a former number of this Journal, gave me additional reasons for distrusting the fashionable theory: though the arguments of that writer in opposition to it partake too much of a speculative character. When, finally, I became acquainted with the valuable experiments and observations of the late Dr. WELLS on dew, and discovered that all the pretended laws of miasm were in fact the phenomena of dew, which latter we could accurately notice, while the separate existence of the former, or its independent action were never demonstrated, I could not hesitate to abandon my belief in a doctrine not supported by fair induction from observed facts, and which, moreover, evidently trammelled the judgment of the physician, both in investigating the causes of fevers and in the administration of his curative means. What is the process of reasoning adopted by the medico-metaphysical school of CULLEN and FORDYCE, and the gossiping one of FOTHERGILL and LETTSOM. Miasm—fevers of an intermittent type—bark and bitters. Thus at one cast we have cause, disease, and remedy, laid down with all the confidence that is asked for an axiom in mathematics, or a table of affinities in chemistry. Miasm working its way into the living body, and poisoning the springs of life, by the production of fever, until after a while it is arrested by the *Vis Medicatrix Naturæ*, and expelled by various outlets, or checked in its career by tonics and antiseptics, would form a good theme for the muse of BOILEAU or POPE; but ought not to have been taught in sober earnestness by a Scotch Professor. How admirably contrasted with this verbiage is the advice of Hippocrates. The man, says

he, who intends to practise physic in such a manner as not to be a reproach to his profession, must have a due regard to the seasons of the year, their different influences, and the several diseases they are capable of producing. He must, also, be well acquainted with the state of the winds peculiar to each country, and the qualities of the waters its inhabitants drink. He must carefully advert to the situations of towns, and the peculiar nature of the country in which they lie, accurately considering, for instance, whether it be *flat and hot, or mountainous and cold*. He must also reflect with what diet and regimen its inhabitants are principally delighted; whether, for example, they are addicted to drinking, gluttony, and idleness, or habituated to various exercises, accustomed to labour, and fonder of eating than drinking. Each of these circumstances is to be accurately investigated and discovered, since by a knowledge of these, the man who is even a stranger to a city or country may become acquainted with its particular nature, and so effectually discover the several diseases to which its inhabitants are subject, that he must be far more capable of prescribing for them than the man who does not sufficiently advert to these matters.

These are important and leading truths, which cannot be too forcibly impressed on our minds: for, unless we are aware of all the circumstances, which can possibly affect or modify the human frame, our practice, even though supported by Greek and Latin lore, and tricked out in all the pedantry of the schools, must necessarily be empirical.

The study of medical geography here assumes its due importance, and greatly facilitates the necessary classification of localities, in which endemics similar in their nature prevail. We shall find that the banks of the Ganges, the Nile, the Amazon, the Mississippi, the Danube, and the Po, though under such different latitudes, exhibit nearly the same grand outlines of endemial disease, subject to the modifications resulting from a higher or lower temperature, whereby, in the former case, fevers assume more of a continued form, with great gastric distress and hepatic determinations, and in the

latter are distinguished by more regular remissions and subsequent pulmonic complications.

But the type of fevers in these countries being more or less intermittent, does not, we think, imply the necessity of admitting miasm for their peculiar and specific cause. Cullen, however, in set terms tells us, that "the similarity of climate, season, and soil, in the different countries in which intermittents arise, and the similarity of the diseases, though arising in different regions, concur in proving that there is one common cause of these diseases, and that this is the marsh miasma."\* A most singular method of argumentation truly! That since given and appreciable causes, (climate, season and soil,) always produce the same evident effects, (intermittents,) we are therefore to refer these latter to an unknown and inappreciable cause, (miasma.) Who does not see that with equal propriety we might admit some unknown principle as the common cause of pulmonic inflammations, because they are met with in similar climates, seasons, and soils, in different countries. To arrive at such a conclusion in either case, demands very different premises from those laid down, as every one must be sensible who gives a common and unit cause for a disease, as contagion for the small pox, which can, we argue, only proceed from this specific agent, because it is a disease met with under the most opposite circumstances of climate, season, and soil.

"The miasma, so universally the cause of fever, is that which arises from marshes or moist ground, acted upon by heat. So many observations have now been made with respect to this, in so many different regions of the earth, that there is neither any doubt of its being in general a cause of fevers, nor of its being universally the cause of intermittent fevers in all their different forms." [*First Lines*.

This general proposition has been enlarged, and attempted to be explained by many succeeding writers, whose opinions have been summed up by BANCROFT, himself a mias-

\* *First Lines*, lxxxiv.

matist of no little zeal and industry. From him we learn that a humid soil abounding in vegetable remains, and acted on by heat, the range of which is from 45 to 100 degrees of Fahrenheit, is the most favourable for the extrication of miasmata. This process is said to go on very slowly, while the mercury continues below 45°, and to be checked when it goes beyond 100°. It is accompanied by an evolution of different gases, chiefly the hydrogen, carburetted hydrogen, and some carbonic gases. VOLTA found that by perforating marshy ground with a stick, gas was disengaged, which, on the approach of a candle, or ignited body, burned with a light bluish flame, and if the holes thus made were numerous, it spread over the surface of the soil. To these changes some have added the diminution of oxygen in the superjacent air; but repeated experiments made in different countries, do not by any means warrant this supposition. We know, on the contrary, that air over a marsh near Milan, contained as much and on some trials rather more oxygen, than that over a part of the ridge of the Grison Alps. To none of the above mentioned gases, nor to any known combination of them, has ever been conceded the property of causing fevers: nor have the minutest and most persevering inquiries, led to the detection and exposure by the senses, of miasm, as a separate principle and cause of fever.

The decomposition of certain plants, as of flax, hemp, and indigo, has, we are told, been productive of miasm and its alleged effects, periodical fevers. Wood decomposed has, on similar authority, been attended with equally alarming consequences.

The assumed origin of miasm as above given, is after all far from being proved by facts. Many persons have resided year after year in the vicinity, I might say on the borders of mill-ponds, which have had their margins in the state most favourable for the extrication of miasm, without their having at all suffered from fever. At times, indeed, we hear of much sickness in such situations, but we shall generally find that fever was at the same time epidemic over extensive districts of country, placed beyond the reach of the mill ponds. The

inhabitants of every Dutch house ought, from the above creed, to be attacked annually with intermittent fevers, since to each is attached a summer house, situated immediately over a small stagnant canal, covered with vegetable remains and exposed to the sun's rays. Here, hours, especially in the evening, are spent by the family, without the members of it being afflicted with disease. In Ireland, the inhabitants of the northern district, where the linen manufacture is chiefly carried on, steep their flax in bog holes and ditches, with the effect of causing a stench for a great distance around, but no fever follows in the train of such a process. Ferguson, in his Essay on Marsh Poison, adduces several facts of a similar nature, to show that miasm cannot emanate from vegetable putrefaction.

Let us examine the subject in another point of view. In northern latitudes, the inhabitants are usually exempt from periodical fever during the winter season, because, forsooth, the temperature of the air is too low to promote vegetable decomposition and putrefaction. But can this reason apply in countries, as in the West Indies and Africa, where the temperature in the coolest season is upwards of 70° Fahrenheit, a temperature most favourable we are told to the production of miasm and its consequences, remittent and intermittent fevers, and yet during this season there is comparative exemption from disease. In summer again, when the heat to which the surface of the soil in these countries is exposed, is often from 120° to 140°, F. we ought not, according to Bancroft, to have miasm evolved, especially when there is great dryness; but it is precisely under such a condition of the atmosphere, that diseases are often most violent and the mortality greatest.

If miasm be the sole, remote, or essential cause of remittent and intermittent fevers, we ought only to meet with them under such circumstances as are admitted to be required for the formation and extrication of this miasm. Now we know from indisputable evidence, that *these diseases have originated and prevailed extensively in argillaceous soils, where no vegetable putrefaction was going on, or at all*

*suspected.\** So much importance did LINNÆUS attach to such a locality, that he wrote his inaugural essay, *Hypothesis nova de februm intermittentium causâ*, to prove that periodical fevers originated in all those places where the soil abounds in clay, and only in such places. VON AENVANK, of Louvaine, has, in the same belief, endeavoured to account for the prevalence of these fevers in an argillaceous soil, by supposing it to possess the property of absorbing oxygen from the atmosphere, and thus impairing its purity. Eudiometrical experiments of HUMBOLDT, and observations of MOROZZI and ROUFFE, are cited as corroborative of this theory, which is not after all sustained by later experience. The facts are, however, not the less cogent, and neither require nor admit the formation or presence of miasm.

*Periodical fevers are met with in mountainous districts where the usually alleged sources of miasm are not seen.*

In the interior of the West India Islands, at an elevation of five or six hundred feet above the level of the sea, amongst a series of mountainous ridges, not directly exposed to currents of exhalations from swampy and low grounds, the form of disease is sometimes intermittent, sometimes remittent or continued, more generally dysenteric or ulcerative.†

Fevers of great intensity make their attacks in the high, hilly, but thickly wooded parts, in the island of Ceylon.‡ On the same authority we learn that places in the island of Sicily, situated on some secondary mountains, lying on the side of the primitive ridge, are very sickly.

*Intermitting fevers have prevailed with violence on dry sandy soils, as in Dutch Brabant, according to PRINGLE; and in a region of Peru, barren from the want of water, and yet nearly uninhabitable from the number of dysenteries and semi-tertians.—(Fordyce.)*

*Intermittents have been brought on by various crude ingesta,§ by cold,|| and by local irritation,¶ without the*

\* Chisholm—Medical Topography of the West India Islands.

† Jackson on Fevers.

‡ Johnson on Tropical Climates.

§ Frank. Senac. Rubini.

|| Alibert.

¶ Giannini.

*persons thus suffering having been previously exposed to marsh effluvia.*

Are we not, after a review of such facts, justifiable by the rules of right philosophy, in denying the correctness of the miasmatic doctrine, and considering as a mere chimera, that which is not formed with any regularity under the very circumstances laid down as appropriate to its evolution, and the absence of which under other conditions does not prevent its alleged characteristic effects from being frequently displayed.

Respect, however, is due to long established opinions, and it will still be but right, in a spirit of courtesy, to suppose that miasm may be formed in states of the soil and air different from those hitherto mentioned, and give rise to fevers of varying violence and malignity.

The epidemical sickness called Inemperies, at Sardinia, is represented as raging from June to September, and as most fatal when there has been a want of rain for four or five months.\*

The air of St. Philip, a town on the western coast of Africa, is so unhealthy on account of the little rain that falls, that the Portuguese used to send their criminals there.†

Will it be affirmed, that in these cases the miasm was extricated in the drying up of the soil, and thereby produced the fever. Unfortunately for such an hypothesis we have the testimony of Jackson, in treating of the West India climate, to apprise us, that in a *dry, rocky and barren soil*, the form under which the endemic appears is usually continued, and if the dry weather be of long continuance, the character which it assumes is frequently malignant and fatal even among such as have been long resident in the country, and are considered as assimilated to the climate. And again, in speaking of Surinam, he tells us, that the rains are frequently excessive; sometimes they fail; the miseries of the colony are then great; mortality among man and beast enormous. "On the whole, says Ferguson,‡ it may be truly said that although

\* Lind on Hot Climates.

† Maffei Sull' Aria, p. 62.

‡ On Marsh Poison.

excessive rains will evidently cause the acknowledged wholesome and unwholesome soils to change places for a time, in respect to health, a year of stunted vegetation, through dry seasons and uncommon drought, is infallibly a year of pestilence to the greater part of the West India Islands."

It is a favourite position with the believers in the miasmatic doctrine, that a very dry and hot season will cause disease, by an evaporation from ponds, swamps, and the borders of rivers, and that when the ground is flooded with rain, health will be restored to the inhabitants. The fever mentioned by Jackson, as occurring in a dry, rocky and barren soil, cannot, one would think, be well accounted for on this principle. But for fear that I should be accused of withholding suitable enigmas to be solved by our oracular miasmatisers, I will mention after Lind and others, that in Guinea during the six or eight months of continued heat, when every thing is eventually hard and dry, and but for the dews would be parched up, and the rivers are restricted to narrow channels, leaving a great part of their beds uncovered, and exposed to the rays of a burning sun, there is no disease. But when the rains begin to fall, the fevers are rife, and the mortality great.

It is very obvious that the origin of the endemic yellow fever, could not be explained on the common hypothesis of marsh effluvia. Hence we find Bancroft endeavouring to show, "that febrific exhalations are often emitted from soils and situations which have no resemblance to a marsh;"\* and Ferguson, already quoted, while he considers "that putrefaction and the matter of disease are altogether distinct and independent elements; that the one travels beyond the other, without producing the smallest bad effect; and that however frequently they may be found in company, they have no necessary connexion," still avows his belief in marsh poison, which "cannot, (he thinks,) emanate from vegetable putrefaction," "but is found most virulent and abundant on the driest surfaces; often where vegetation never existed, nor *could* exist for the torrents, such as the deep and steep ra-

\* Sequel, p. 254.

vine of a dried water-course, and that it is never found in savannahs or plains, that have been flooded in the rainy season, till their surface had been thoroughly exsiccated; vegetation burnt up, and its putrefaction rendered as impossible as the putrefaction of an Egyptian mummy." He thinks this said febrific miasma very certainly generated from the paucity of water where it has previously abounded, provided that paucity be short of actual dryness. To the production of this a high atmospherical temperature is indispensable. "It would," he adds, "be unphilosophical to suppose, that the marsh poison, because other distempers, such as dysentery, coexist with it, ever produces any disease but the specific one of which it is the acknowledged parent, varying, however, in form, and as a modification of effect from the same cause, from the common ague of the fens of Lincolnshire, through all the milder remittent types, up to the aggravated yellow fever, or malignant remittent of the West Indies; and that variation so certain and uniform, in proportion to the power of the remote exciting cause, that the varying types of fever might be measured almost to a certainty by the degrees of solar heat, as marked by the thermometer."

The reader is hardly prepared for such a conclusion, from the premises laid down by our author, and some of his brother miasmatisers might object to his considering heat as the remote exciting cause, (if such phraseology be admissible,) rather than miasm. But our business at present is with the first part of the proposition, which insists on the unity of cause—miasm, and sameness of effect—remittent and intermittent fevers, and a denial that dysentery or such like co-existing diseases are to form a link in the chain of causation. The reverse opinion would seem the more correct, since we find this disease prevailing at the same season, and in the same district of country with periodical fevers, and at times alternating with them, and even assuming their type.\* These facts are of such general notoriety, as to have induced the

\* Pringle, Cleghorn, Blanc.

late Dr. Rush to speak "of the unity of bilious fevers and dysentery," and led the confirmed miasmatists to insist on the latter having the same marsh origin as the former. The admission is in a manner unavoidable. But when we can show that the disease in question does frequently and avowedly originate from atmospherical vicissitudes and extremes, aided by the use of crude ingesta, and exhibits such a close propinquity to remittent and intermittent fevers, are we not justifiable in receiving the same common causes for the production of the latter as of the former, and consequently in overlooking or denying the necessary agency of marsh miasm in either. To a miasmatist himself I will appeal for a confirmation of this view. "Of the *remote* causes I need say little. They are the same in all parts of the world, atmospherical vicissitudes. Perspiration and biliary secretion being in excess during the intense heat of the day, are so much the more easily checked by the damp chills of the night; and the consequences which ensue are clearly deducible from the principle I have stated. In short, the same general causes produce bilious fever, hepatitis, and dysentery. They are the three branches from the same stem, the organs principally affected occasioning the variety of aspect."\* We prefer this language to that of Jackson, who, in treating of the West India climate and diseases, tells us, that "the action of the morbid cause, (endemic irritation,) instead of being what is commonly called febrile, is not unfrequently *dysenteric*, sometimes eruptive and ulcerative on *dry, bare, rocky and hilly positions near the sea coast, or in positions where water flows with a rapid course.*" Both these writers are in accordance with the others already cited, (and the list might be much swelled,) as to the general sameness of causes of fever and dysentery. But how can there be general sameness or close affinity, if a peculiar specific agent, miasm or endemic irritation, be essential to the production of the former, and not necessary to that of the latter. Will it be alleged that the features of fever are so distinctive and so character-

\* Johnson on Tropical Climates, Part II. sec. ix. Dysentery.

istic as to force our belief in a specific cause. The argument we apprehend, if not stronger, will lose none of its force, if applied to the ætiology of dysentery. Indeed we must suppose that it was the somewhat peculiar nature of the symptoms in this disease, which induced physicians at one time to consider it as contagious.\* The revolution of opinions respecting this malady, has finally brought us round to the earliest entertained and most natural belief. Ought we not, if we are inclined to use equal industry in observation, and independence in judging, to arrive at a similar conclusion in the study of the causes of periodical fevers.

As one of the numerous and troublesome family, bilious fever stands foremost in these United States. It is the gastric or bilious remittent of Jackson, and may, as we have already seen, prevail where no miasmatic origin can be detected. "This," says Johnson, "is the grand endemic, or rather epidemic, (*morbus regionalis*,) of hot climates; and although greatly allied in many of its symptoms, perhaps generally combined with the marsh remittent already described, yet it occurs in various places, both at sea and on shore, where paludal effluvia cannot be suspected." This is but confirmatory of what is held by this gentleman when speaking of dysentery: and yet both are by him couched under the head of "specific diseases," for what reason we are at a loss to discover.

The yellow fever is now very generally considered as the product of miasm, but whether of a more subtle and malignant kind, or of increased activity by the high accompanying temperature, is a point on which writers are not well agreed. One thing, however, connected with this question, seems to be pretty generally admitted, both by contagionists and miasmaticists, viz. that this fever may be caused by heat as a predisponent, and any imprudence in regimen as an exciter.

Among the physicians of the first class, Sir GILBERT BLANE

\* "And upon the whole it is probable, that a specific contagion is to be considered as always the remote cause of this disease," (dysentery.) *Cullen, First Lines.*

is no minor authority. In his letter to Mr. RUFUS KING, then Ambassador at the court of St. James, I find the following. "After laying together and considering fully all the facts relating to this subject, it appears to me that the yellow fever cannot be produced but in a season or climate in which the heat of the atmosphere is pretty uniformly, for a length of time, above the eightieth degree of Fahrenheit's thermometer; that under the influence of this heat, Europeans newly arrived, and more especially in circumstances of intemperance or fatigue in the sun, may be subject to it in many instances, but that it has usually become general only by the previous influence of that infection which produces the jail, hospital, or ship's fever, or from the influence of putrid exhalations; and that when so produced it continues itself by infection."

Most cis-atlantic physicians are disbelievers in the doctrine of infection as here laid down, and would prefer accrediting the effects of heat as above given. These effects are further evidenced by the facts detailed in the work of this author on the Diseases of Seamen, in which, after speaking of that sudden and fatal affection called the *coup de soleil*, produced by the direct rays of the sun, and the tendency of this heat when associated with fatigue and intemperance to bring on the fatal diseases of newly arrived Europeans in the West Indies, he adds:—"This has been remarkably conspicuous in the years 1794 and 1795, during which the most deplorable ravage ever known was made in the great armaments sent to the West Indies, yet the prisoners of war remained exempt from it, according to the testimony of those who had the custody of them at Jamaica and Antigua. There can be no doubt that the peculiarity of situation to which this is principally imputable is shelter from the sun," p. 203, London, 1803. It is pleasant to observe the records of an observing mind, given thus explicitly, at variance with the spirit of system and closet abstraction, and of his own theory. Did it occur to Sir Gilbert that the above facts are among the strongest that could be furnished in opposition to his notion of yellow fever becoming general "by the previous influence of that

infection which produces the jail, hospital, or ship fever,"—since the prisoners above alluded to must have been much exposed to this influence, whether they were confined on board a ship, or in durance on shore. Will "the influence of putrid exhalations," or of miasma afford any better solution? Not until it be proved that prison-ships and prisons are more favourably situated than ships in actual service or barracks.

Let us next hear the sentiments of the miasmatists on this head. "I am far from presuming to deny, says Dr. Ferguson, that there are fevers from pure excitement; for soldiers and others have been attacked and died of yellow fever, before they landed in the West Indies, or could be exposed to the influence of land miasmata in any shape." As this writer is neither ignorant of, nor inclined to disavow the occasional origin of fever from impure air generated in the holds of ships, we may presume no such cause was present in the above-mentioned instances. MOSELEY was, we know, of the opinion that the yellow fever was a calenture or disease the offspring of heat. DICKSON, in his valuable essay on this disease, declares, after much experience of its origin and nature, that marshy effluvia or similar impure emanations, in other situations, are a great source of yellow fever. "But whatever may be the peculiar coincidence of circumstances or modification of cause, most fertile in the generation of yellow fever, an uniformly high temperature is the *causa sine qua non*. This is literally and eminently entitled to be so denominated, because it indispensably precedes the effect." And in another place. "The general healthiness of the West Indies, as well as of particular islands, varies considerably in different years, and at different periods. It is liable to be affected by certain states of the air, as unusually wet, or dry, or close, or otherwise unseasonable weather for the time of the year, by calms, by variations, (especially to the southward,) from the usual trade winds, and in the quantity of the electric fluid, and in certain years by what has been termed "an epidemical constitution of the atmosphere."

The island of Barbadoes is clear of wood, the land is moderately raised above the level of the sea, and every spot is

cultivated; there are but few swamps, and those are inconsiderable, and some rivulets only occasionally swelled by rains.\* Notwithstanding this, yellow fever is, according to Jackson, more common in the hospitals of Barbadoes than in any other of the British islands. Be it remembered, however, that Bridgetown, the capital, and its vicinity are extremely hot from June to November: the thermometer, at noon varying from eighty-four to ninety degrees of Fahrenheit in the shade. What follows is not susceptible of ready explanation by the miasmatic theory. "The parallel of health, says Dr. M'ARTHUR, between the army and navy is worthy of notice. The fever for some preceding years has appeared in both about the same time, and attacked men of similar habits; but has in general been more aggravated on shore than at sea, or even on board the ships lying in Carlisle Bay." The causes of the malady are by him considered as a peculiar habit, consisting, in a disposition in the European constitution, to take on inflammatory action. "This disposition is excited into action by a variety of causes, the chief of which are intemperance, excessive fatigue in the sun, perspiration checked by being exposed to a current of air, or sleeping exposed to the dews, costiveness, &c."†

Mr. Dickinson, whose ample experience as a Staff Surgeon in the West Indies, entitles him to be heard with respect on this question, gives the following as the causes. "The predisposition consists in an inflammatory diathesis—an aptitude to diseases of general increased excitement; this appears sufficiently manifest by a consideration of the subjects, (strangury from temperate regions,) already stated as exclusively liable to its attacks. The exciting cause is an exposure to solar radiation, while unaccustomed to its influence, and unprepared to resist the force of its impression by the adoption of preventive measures. The effect of heat is liable to augmentation, if accompanied by violent exercise, by full living, and intoxication."‡

\* Account of the Causus, or Yellow Fever of the West Indies. By Dr. M'Arthur, M. D. &c.

† Johnson on Tropical Climates.

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‡ Johnson, Op. Cit.

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The prevalence of a particular wind, whether it blow over land or ocean, will often give additional force to solar heat, and prove a powerful contributing cause of fever. Sometimes this effect is dependent on peculiar properties of the wind, sometimes on its imperfect power of perflation, and thereby giving rise indirectly to a stagnant or stationary atmosphere.

LEFORT advances, with some confidence, his belief, that yellow fever need not be apprehended in some of the West India Islands, unless the winds from the southward have prevailed sometime previously. And BANCROFT in his inquiry into the epidemic of Gibraltar, in 1804, suggests, as "obvious and prominent causes, the accumulation of decomposable matters within the town, *and the long prevalence of a dry and scorching east wind*, which produced a very high atmospheric temperature, without any salutary ventilation of the place, as it was completely obstructed in its course by the high mountain behind the town, *in and over* which the air was for many weeks nearly stagnant. A similar dry and scorching east wind, blowing with too little force to change and purify the atmosphere, has invariably preceded and accompanied every recurrence of the yellow fever at Cadiz and other cities of Spain; and its effects in the year 1804 were very extensive and remarkable." FELLOWES, TOWNSEND, and BURNET, bear testimony to this fact; and, it is added, that at Cadiz, easterly winds, which generally preceded epidemics, occasion very distressing feelings even when their effects do not amount to disease.

I am really unable to discover the advances we make in philosophical induction by denying the direct agency of heat and stagnant atmosphere in the production of disease, and insisting in preference on the evolution of miasmata in these circumstances. To select as a cause that which cannot be detected or appreciated in any other way than by alleging certain well known phenomena as its effects, is not a novel process in scholastic argumentation; but ought not to obtain much favour at the present day, when mysticism has ceased to be in vogue, or to have any power over the sciences and liberal arts. That epilepsy and insanity are penalties incur-

red by guilt, and dispensations of wrath from the Maker to the creature, is an opinion occasionally advanced by those who are expounders of sacred writ, and one which we may at times be compelled to hear with deference, though we cannot yield it conviction. But, that there is such a thing as miasma, or malaria, as a substantive cause of fever, is one of those transmitted dogmas, which has not even the venerable support of the old man of Cos, nor of those who from time to time have become classical by following in his steps. Homer himself, with all his machinery of supernal powers, presents us with no being of such equivocal origin as this, which can only be compared to another poetical creation, Death that at birth—

“Forth issued, brandishing his fatal dart

“Made to destroy.”

Or more aptly it may be supposed from chaos sprung, and to chance, “high arbiter,” submissive.

This eagerness to reduce the many operative agents to one principle, which is to explain all the phenomena of the subject or the science, is not confined to medicine alone. Sensation and association for the metaphysicians—phlogiston at one time and oxygen at another for the chemists, have been found equally as pliable causes of the operations of mind, in the first case, and of the changes of matter, in the second, as *putrid ferment*, *anima medica*, *vis medicatrix*, *vital principle*, *contagion*, and last, though not least, *miasm*, to account for the production of diseases, or protection from their influence. All these baseless fabrics vanish before the fixed look of scrutiny, or if the mental vision occasionally tolerate their presence, it ought to be in one of those fits of voluntary illusion in which the imagination at times loves to sport, and for a while gives “to airy nothings a local habitation and a name.”

The reader, whatever may have been his previous prepossessions or prejudices, will, I trust, have seen in the facts and arguments already given wherewithal to destroy, or, at least, greatly diminish, his faith in the doctrine of miasm. The more fully to complete this object, I will now give with consistent brevity a sketch of the effects on the human body of the cir-

cumfusa. Some apology might be due for adopting this course, if a knowledge of all the physical agents by which we are surrounded, and by which we live, and through which we so generally become diseased, were made, as they ought to be, a branch of elementary study, and the basis of ætiology.

*Of Heat.* Solar heat acts primarily on the skin, and by means of the air on the mucous surface of the lungs; and like every stimulant applied to these surfaces, excites the nervous and capillary expansion, and subsequently the brain, heart and arteries. There is increased cerebral activity and hurried circulation, both of which are moderated by the coming on of perspiration. If the heat be long continued, the evaporation is disproportionately rapid for the supply of fluid from the secretory vessels of the skin, which is affected with dryness and redness, followed by induration and thickening, and after a time it assumes a brownish yellow colour.\*

So far the effects are nearly physiological, and the person may become gradually inured to heat, without any great inconvenience or suffering; but its prolonged and more concentrated application is apt to bring on a diseased or pathological state, evinced by a parched and dry skin, *a burning sensation at the stomach*,† hurried respiration, and finally, (as sometimes occurs in the great desert of Africa, during the prevalence of the Semoom or Samiel wind,) asphyxia, hæmorrhage from the nose and lungs, and death.‡ The more chronic state caused by heat, shows itself in loss of appetite, faintness, nausea, deficient and irregular secretion from the mucous surfaces, and phlogosis of these membranes, hence the heat and tenderness of the stomach and intestines, and the parched and furred tongue. The liver, like all other glands, is obedient to the stimulation of the mucous surface, on which its duct opens, and secretes bile in increased quantity, which fluid aids in bringing on the succeeding phenomena of vomiting, diarrhœa, and griping pains. Like other glands also, it sympathises, or takes on synchronous morbid action with its corresponding surface, and hence its secretion is a

\* Blumenbach, p. 110.

† Travels of Ali Bey.

‡ Volney

fluid imperfectly elaborated, an irritant to the gastro-enteric surface, already irritated and prone to inflammation. It is needless now to inquire, whether this irregularity in the hepatic system, result from direct sympathy with the skin, or mediately with it by means of the intestinal surface. In either case heat is the exciting cause. On occasions, and those of no unfrequent occurrence, it will act as a predisponent, by keeping up such irritability as to render the external and internal surfaces liable to be readily affected by other circumfusa or various ingesta.

If the skin be exposed to the prolonged irritation of heat, eruptions, such as urticaria, pustules, scabs, leprosy, and pellagra supervene.

Heat and light combined, are a common cause of ophthalmia in warm climates, as in Egypt and Syria, where animals are subject to this disease; also in other parts of Africa and Asia, and southern Europe, particularly where sand and white rocks reflect the sun's rays. A complaint is described by PROSPER ALPINUS, which he attributes to the heat of the Egyptian climate. It is marked by excessive thirst, not always accompanied by fever, and often occurring in the midst of perfect health: the repeated fainting fits in it would soon terminate life, unless water were supplied to the full wish of the sufferer. If this be wanting, the patient dies in a swoon, or falls into a hectic fever. But as this malady may be attributed by some ingenious mystics to miasm, I shall next notice one, avowedly the direct consequence of undue exposure to intense solar heat. Its names are various—Insolation, *Ictus Solis*, *Seiriasis*, *Coup de Soleil*, Sun Stroke, &c.

The more moderate grade of this disease is evinced in the delicate inhabitants of a city, who have been long shut up in their apartments, and are, on the approach of spring, exposed to the sun. They suffer then from general headache, or acute pains in the forehead and temples, redness and dryness of the face and eyes, dry heat of the skin, somnolency or anxiety which prevents from sleeping, and sometimes distinctly marked fever. But sun strokes are much more dangerous in summer, at which season labourers in the field, ma-

sons, tilers, and others, who work out of doors, soldiers who make long and painful marches exposed to the solar rays, are the greatest sufferers. The greatest mortality on record from this cause, is that which occurred in Pekin in the year 1743. Eleven thousand persons fell dead in the streets from the insufferable heat, between the 14th and 25th of July. The effects of insolation are acute pain in the head, the vessels of which are gorged with blood; the eyes are very red and watery; the pulse hard, quick, and irregular; spasmodic and other convulsive movements and bilious vomiting supervene; the urine is suppressed. At other times the head is swelled in an extraordinary degree and covered with erysipelas; the subject falls into paralysis or subsultus tendinum, and the disease has a rapid termination. Chronic cephalalgia, maniacal delirium, and fatal phrenitis, are occasionally the distinguishing traits.

Examinations after death show the cerebral vessels much enlarged and gorged with blood; at times there is an extravasation of this fluid in the cerebral cavities, and at others no trace of disease perceptible.

The belief in the pernicious effects of exposure to the sun's rays, will perhaps account for the narrowness of the streets, so general in all the cities of southern Europe. We learn the importance which the Romans attached to this agency, from a passage of Tacitus, where he attributes the unhealthiness of Rome, after the great fire of Nero, to the streets being wider on the rebuilding of the city, and consequently not affording the same shelter as formerly from the sun's rays.

The direct and immediate effects of heat are increased by a dry atmosphere, while the predisposition to disease, and ready liability to be acted on by common and morbid causes, are greatest in a hot and moist air.

"It is remarkable," says Pringle, "that pestilential diseases have frequently occurred in dry and hot summers; and agreeable to this I have observed that the most sickly seasons have been attended with the greatest heat and the least rain." He quotes BARTHOLINUS's account of the fever at Copenhagen in 1652, which began in autumn, after an unusually hot

and dry summer, and which, in its symptoms and seat, was similar to our yellow fever. The stomach and duodenum were always inflamed or mortified, and Bartholinus hence concluded that these parts are the seat of all malignant fevers.\*

I have already given the experience of Jackson, as to the great mortality during dry weather, and in a dry, rocky and barren soil.

The great number of deaths in our large cities during the intense dry heat of last summer, needs but to be alluded to on this occasion. We may be told that it was not fever which produced this result, though I cannot really see what consolation the friends of individuals who were carried off would derive, by being told that death was caused by insolation and not miasma—or by intense heat and cold water, rather than by miasm and wine or brandy. To me, thirty or forty persons carried off in the course of a week, some exhibiting all the marks of apoplexy or congestion, and effusion in the brain, and others of a violent spasm or colic, and inflammation of the stomach and bowels, evinced the operation of causes, to the full as potent as five, eight, or ten deaths in the same time by yellow fever, the alleged product of miasm. Is the suddenness of the event, and the uncommonly acute character of the first mentioned cases, proceeding from well known causes, a sufficient reason for overlooking them in the study of ætiology, merely that we may dwell with complacency on the latter, more regular in their course, and therefore to be referred to some unknown agency?

During the last summer, I was called in the morning to visit a man in Little Water street, near South, whom I found in all the horrors of cerebral congestion and convulsions. In an hour afterwards he was a corpse. He had retired to rest the preceding night after a supper of mush and milk, and a pediluvium of cold water: and it was not before early morn

\* Here was a hint for Broussais, rather more significative than many expressions of Baglivi, to whom the French reformer is said to be so largely indebted.

that he awoke persons in the room beneath him by delirious efforts, as if he wished to bail out a boat. He was a fisherman, and of intemperate habits: he had been much exposed to the sun during the preceding week, and had on two different occasions suffered from a sun stroke. His mind had been troubled some time previously by domestic grievances. His habit of body was full and muscular. On examination, the brain was found to be the part chiefly diseased. The arachnoid membrane was thickened and covered with fluid, which also filled the ventricles. The pia mater was injected in patches, and its larger vessels turgid. The stomach and small intestines were deeply injected in different parts, and the liver much enlarged. Shall we admit as causes of disease and death in this case, insolation, intemperance, and mental anxiety. Are they not, by the showing of any side, causes of sufficient power, and adequate to the production of the above marked symptoms and appearances on dissection.

A week after the event just recorded, it became my duty to attend a man in Water street near Walnut. He was then in the seventh day of his disease, and during the three following days it exhibited the distinctly marked characters of yellow fever, to such a degree as to leave no doubt in the minds of the several physicians who from time to time came to see it—colour of the skin and eyes, singultus, vomiting of dark fluid, mixed with coffee ground particles. On the two days preceding his death he had repeated attacks of convulsions at intervals of nearly every four hours.

On examination after death, in the presence of eleven medical gentlemen, the following were the appearances: muscular system and adipose tissue full: dura and pia mater both tinged yellow, the vessels of the latter gorged; slight adhesion between the hemispheres; the ventricles contained a small quantity of yellow fluid. Much bloody serum issued from the spinal canal on removing the brain.

The trachea was covered with a brown mucus, which, when removed, exhibited the lining membrane of a dark mahogany colour, without any polish—an appearance like this ex-

tending down to the lungs. In the abdomen, the liver and intestinal canal were greatly altered; the former was of a pale yellow, firm and small. The cardiac region of the stomach varied in colour from a bright red to a livid. Its contents were a half pint of thick dark fluid, nearly homogeneous. The small intestines throughout were coated with a dark mucus, which when removed left to view a highly injected surface. The arch of the colon was of the same appearance.

The subject in question had been a shoemaker, twenty-three years of age, and addicted to the free use of ardent spirits.

Here, of course, by the received doctrines, we must regard miasm as the cause of the disease. About as satisfactory an explanation as to say that the first mentioned case was disease from an epidemical state of the air, or from endemic irritation.

*Of Moisture.*—Simple atmospheric moisture, without any great extreme of temperature, is not found to be productive of bad effects on a healthy subject conforming to the rules of hygiene. It gives, however, greater activity to the operation of both heat and cold; and is, when conjoined with either, a frequent cause of disease. Humidity acts on the skin by diminishing exhalation and absorption, and producing an atony of the extreme vessels. The circulation participates in this languor. The thirst is less, and the secretion of urine more abundant. Respiration is laborious; a kind of oppression is felt, owing probably to the watery vapour not being completely expelled from the lungs; the air which served for inhalation having been already charged with humidity. The secretion from the mucous membranes is usually augmented, hence a species of coryza and even diarrhœa. The senses are usually more obtuse in a humid air, owing to the undue moistening of their surfaces. Sensibility in general is blunted, muscular power diminished, and all the movements of the body slower and less agile.

TOURTELLE in his Hygiene, assures us that a humid state of the atmosphere is unfavourable to vegetable as well as animal life, and alters strangely the fluids and secretions. In a

wet spring the flowers of the yellow laurel rose, (*ægolethron*,) are poisonous, and the honey which the bees extract from them has similar noxious properties.

Moisture joined to cold constitutes one of the most efficient agents in the production of scurvy, scrofula, cynanche tonsillaris and trachealis, catarrhal fevers, and rheumatism. It is often the exciting cause of diarrhœa, dysentery, and tetanus, the more especially if there has been preceding great heat. In northern latitudes there is no condition of the air so invariably pernicious, so chilling and oppressive to the organs of respiration as the combination of frost with fog. It is this state, which has been found to accompany, if not produce, extensive influenzas and wide spreading pneumonic diseases.

In southern countries on the other hand, the union of heat and moisture may be viewed as exerting an influence paramount to that of all other causes, whether it be between decks of a ship, in close barracks, or in the lower parts of a city, and the swamps and low grounds in the country. This condition of the atmosphere acquires greater intensity of effect from the calms, or little circulation of air so often accompanying it. There is in fact a stagnant atmosphere which needs no miasmatic aid to give it destructive power. Moisture and cold, will, if ventilation be not attended to, produce scurvies on board a ship. Moisture and heat, with equal negligence in ventilation, will, in the same situation, be followed by remittent or gastric fevers. Let us transfer the question from sea to land. Along the shores of the Baltic scurvy is the chief endemic. Along the shores of the Mediterranean fevers are the regular diseases. If miasm be insisted on as the essential predisposing cause of the latter, why not dwell on the necessary agency of a like poison to the production of the former. We shall then have a febrific miasm and a scorbutic miasm.\* And by a little extension, I was

\* I was not aware, when I spoke thus of scurvy, that the spirit of generalization, had gone so far as to attribute it to the same common cause as intermittent fevers: but I now find, that Montfalcon in his *Histoire des Marais*, &c. has actually advanced this notion!

going to say perversion, of ingenuity, we may have an apoplectic miasm, and an arthritic miasm. This fictitious substantive may, in fine, be joined to as many appellatives as there are varieties of diseases, with the great advantage of easy parlance, and still easier thought, as no ideas can be affixed to such language.

*Of Cold.*—The effects of continued cold on the inhabitants of a country are well displayed in the Laplanders and Esquimaux. Their height is below the common standard; their organs are in a state of imperfection, similar to that of infancy; their external senses dull and sometimes obtuse. The operation of cold on the body generally is of a decidedly sedative nature, though when speaking of its morbid effects, we must generally hold in mind that these latter are compounded of the first application of cold, and secondary one of heat, and other stimuli. Still we are justifiable in speaking of cold as producing such and such effects, since it is the *causa sine qua non*. On the surface of the body, cold, in extreme, causes chops, chilblains, and even gangrene of the extremities—fingers, toes, ears, nose. When continued for a still longer period it strikes at the sources of vitality. The torpor of the capillaries represents that of the larger vessels, the heart and the brain. There is lethargy and apoplexy, particularly venous from the congestion of blood, and hence the great and at times irresistible propensity to sleep, in those who are exposed to great cold. We are not to conceive of a reflux of blood to the great cavities or the internal surfaces from cold outwardly applied. The torpor of the former immediately succeeds or is synchronous with that of the latter: and the reaction in both is simultaneous, or with little interval. The rest of torpor and the excitement of reaction may then be said to constitute this state of things. What is seen to take place in the skin is repeated in the other surfaces, mucous, serous, and synovial; and the alterations in the one will serve to give us an idea of the changes in the others, which cannot however, we know, be effected with equal impunity. The skin after exposure to intense cold, may speedily recover the rhythm of its functions: but not so with the internal sympa-

thizing membranes, lining or investing, which evince their disorder in catarrhs, pleurisies, rheumatisms, anginas, &c. The blush of the skin, heated after being immersed in cold water or long acted on by cold air, is often no more than sensation. The same blush in the parts already specified is irritation and pain, productive of fever.

Habit will reconcile the system to extreme cold, and even to certain alternations: but unaccustomed exposure, as in the case of the inhabitants of warm climates, during a northern winter; or in temperate climates, the prolongation and unusual severity of this season, and the habit weakened by prior disease or old age, place persons in the situation most liable to suffer from it. The appalling examples of the physical and moral influence of cold in the disastrous retreat of the French army from Russia, are too recent and too generally known to require particularizing here. It is not perhaps as generally understood, that the loss of the Russians was not much inferior during their advance. Observe now the power of theory over the explanations of the phenomena of disease. In Egypt it was not thought possible for a few thousand French and English soldiers to die without the aid of miasm or contagion, or both, while in Russia, hundreds of thousands of French and Russians were allowed to make their final exit in every variety of torment, by the common place agencies of cold and starvation.

In England and France it has been remarked, that the very cold winters have been the most productive of disease. BLANE, HEBERDEN, and BATEMAN, among others, have, in opposition to the vulgar idea of the salutary nature of cold, and the comparative health in winter, exhibited the number, complication, and violence of epidemics in this season. Records of similar visitations in these United States are by no means unfrequent, whether we call them putrid sore throats, spotted fever, cold plague, typhus pleurisy, pneumonia typhoides, or influenza.

Those who think to draw a distinction between fevers, the alleged product of miasm, and the phlegmasiæ resulting from acknowledged atmospherical vicissitudes, in the state of the

digestive system and appearance of the tongue, as indicative of the former, will be egregiously deceived in their prognosis. Autumnal fevers will in their decline be often associated with pulmonic disorder, in consequence, as we are sagely told, of the miasm or endemic irritation directing its force to the pleura and lungs in place of the stomach and liver. Winter fevers, the influenza for example, will begin its attack like catarrhal or pleuritic fever, and in its course attack the chylipoetic viscera. During the last winter I have seen the disease travel from the head and breast, when it showed itself as coryza and catarrh, to the stomach, causing vomiting, and the intestines producing dysentery. Authorities might, if necessary, be largely furnished to corroborate this view.

*Of Winds.*—Though a wind blowing from a particular quarter, or one suddenly succeeding another, seems often to have no effect distinguishable from that which may be explained by the accompanying temperature, or its sudden change; yet at other times, unquestionably, there is something inherent in the wind itself, which exerts a powerful impression on the animal frame.

In the northern hemisphere, we are much more sensible to the same thermometrical measure of cold when a northerly wind blows, rather than when it is calm; and during the prevalence of a southerly wind, that heat, otherwise tolerable, becomes oppressive. In a given temperature our feelings are widely different when we are fanned by a westerly, and blown on by an easterly wind. Nor can this difference be accounted for from the operation of local causes, as the intervention of land or water. An east wind, says Bateman,\* with the thermometer at 50°, F. will impress the body with a more chilling effect than a south-west wind, when that instrument indicates a temperature ten degrees lower. The Edinburgh Reviewers might, perhaps, explain this fact, on the supposition of the east wind being the bearer of miasmata from Holland to London, across the German Ocean. Let not the serious reader accuse me of undue exaggeration of the extrava-

\* On the Diseases of London.

gance of the miasmatic theory, by feigning for a moment its advocates to entertain such phantasies. This very notion of the migratory tendency of marsh air is gravely advanced in the forty-fifth number of the Edinburgh Review. We must do the writer the justice of acknowledging, that he is not always prone to give his favourite such ærial journeys, for in the same article, he describes, with becoming minuteness, its creeping progress along the parks, and through streets in the west end of London. Wo be to you, ye votaries of fashion, who resort thither to display your fine equipages, fine dresses, graceful airs, and pretty faces. Worse than the cup of Circe is presented to you in the subtle miasm, which moves along with your chariots in Hyde Park, and follows in your train in Bond street. Little do you dream of your danger, or imagine that, while you are swallowing a compliment or a jelly, you are at the same time receiving a dose of this poison. Scornful persons and sneerers ought to be particularly careful how they indulge their propensities, since upturned noses offer a fair passage to this insidious miasm, which, if it cannot travel downwards to the stomach, flies up incontinently to the brain, producing phrenitis or apoplexy.

Fond as the North American Review is of echoing the opinions of its trans-atlantic compeer, I am afraid it will hardly venture on the experiment of conducting the miasm across the ocean, and make it the cause of the rigors, chills, rheumatisms, hæmoptyses, and fevers, not to mention the dyspeptic band, of all which the easterly wind is, in this country, a munificent supporter. We are usually taught that the penetrating chillness and cold, of which this wind is the bearer, arise from its blowing off the ocean; but unluckily for this easy and conversational explanation, the finest weather in Great Britain, during the summer months, is when the south-west wind prevails; that, be it remembered, which blows from this same ocean, the alleged origin of the malignity of the easterly. Need we mention, that the wind most pure and invigorating to us on this continent is the westerly or land wind. Now comparing the Atlantic ocean to one vast morass, and supposing all to the eastward of it *terra incog-*

*nita*, we should, by the received theory, most strenuously argue that all the disorders above mentioned were produced or kept up, not by the wind sweeping over it, but by the miasm arising from it, and of which the wind was the conductor. Who does not see that many of the histories of reputed miasmatic diseases rest on no better foundation than is furnished in this suppositious case. Take the following as an instance of the remarkably different effects of two winds, without miasmatic aid having been invoked for either. In the account of the influenza, which prevailed in England in 1803, by Dr. CARRICK, we learn that the inhabitants of that side of Richmond terrace on Clifton Hill, near Bath, which fronted the east, were universally attacked with the disease, while on the south side, the great majority both of persons and families, in all other respects similarly circumstanced, escaped it entirely. When BACON repeated the proverb of the east wind being good for neither man nor beast, and the school of PARACELSUS, in giving niches to the north, south, and west winds, in the temple of Juno, or the air, denied one to the east, they were not aware that the source of its mischievous tendencies was miasm from marshes, low grounds, or all grounds over which it might blow. The notion would have chimed in well with the extravagant genius of Paracelsus. Bacon might have very provokingly asked for proofs; unless, indeed, he had put it on the same footing as judicial astrology, in which we are told he suffered his great mind to believe.

The noxious effects of an easterly wind would seem to be alike potent, whether heat or cold be associated with it. Of its having been regarded as one of the main predisposing causes of the Gibraltar and some other Spanish epidemics, I have already spoken. Lind's remarks on its influence in England are not less expressive. "An east wind," says he, "is usually accompanied by a cold, damp, and unwholesome vapour, which is observed to affect both animal and vegetable health, and in many places to give rise and obstinacy to intermitting fevers, as also to produce frequent relapses." In eastern China, the north-east monsoon induces all the un-

pleasant feelings that distinguish it in our own country; and it has the same property of bringing on attacks of intermittent fever which distinguish it in England. When a convalescent from this disease in Macao, I was immediately sensible of a change of wind from the south-west to the south-east, though I was in the house, and the thermometer was not then affected.

The *south wind* displays features not less characteristic than the one just treated of. It is the associate of a moist and rarefied atmosphere, and has a singularly relaxing and enervating effect. It so generally brings rain, and the north clear weather, that the poets when they describe the deluge, feign the latter to have been at that time imprisoned, and the former sent out with a very extensive commission.

The south wind is emphatically the companion of pestilence, whether it be the semoom or samiel of the desert, sweeping across Egypt, like the destroying angel, or the milder austral breeze, striking on the coasts of Attica, and bearing, as Thucydides thought, the seeds of the plague, which ravaged Athens in the Peloponnesian war; or that sung by the Mantuan bard, in the Georgics, lib. i. l. 444.

Arboribusque satisque, Notus pecorique sinister.

Or more celebrated in Homeric verse, when,

—— Vapours blown by Auster's sultry breath  
*Pregnant with plague*, and shedding seeds of death,  
 Beneath the rage of burning Sirius rise,  
 Choke the parched earth and blacken all the skies.

CÆLSUS, in giving advice for the preservation of health, during the prevalence of pestilential diseases, insists particularly on its observance, when they are excited by a southern wind. Great stress is laid by VITRUVIUS on the situation of cities and houses, in reference to their exposure to particular winds. In speaking of Mitylene, he tells us that when the south wind blows, men become sick; when the north-west, they cough; when the north prevails, they are restored to health. Would this account be any more lucid, or applicable to the preservation of health, if Vitruvius, with something like

Egyptian mysticism, had told us that people became sick when the south wind blew, because it carried miasm, or that they coughed because the north-west wind had in it a pneumonic principle. I doubt whether a modern miasmatic commentator could add an iota to elucidate the plain aphorism of Bacon, *Flante Austro, pestilentes morbos grassari*.

In this western hemisphere, the southerly wind will be found to bear the same character as in the old world. I have already quoted Dr. LEFORT's opinion on the subject of its producing and keeping up the yellow fever. They who have read some of the many accounts of the sirocco wind in the Mediterranean, have been taught to believe that its peculiar effects arise from its blowing first over the African desert, and holding a certain portion of sandy or other particles in a kind of suspension. But in the West Indies, on the testimony of the author just mentioned, we learn that the sirocco is not the wind of the desert, but the south wind, whether from land or ocean. "This action of the southerly winds," says Lefort, "was sensibly felt by all animated nature in those islands, and produced indefinable effects on the senses: it was felt by the man in his bed, or seated at his desk: it oppressed, prostrated, and urged to the darkest melancholy."

*North wind.* Under this head I include those winds which blow from the north-west to the north-east; dry, clear, cold and invigorating from the former quarter; hazy, cold, and penetrating from the latter.

Celsus, echoing the language of Hippocrates, considers the north wind as exciting cough, irritating the fauces, constipating the bowels, and inducing difficult urination and pain in the side and breast, with shivering. In fact, anginas, catarrhs, rheumatisms, pneumonic and pleuritic affections, are the legitimate products of the winter season, and long continued northerly winds. When these alternate with moisture and occasional southern blasts, the thoracic affections become complicated with abdominal ones, and we have fevers of various kinds. The thermometer low and barometer high, indicate a state of atmosphere favourable to the production of inflammatory disease; while the barometer low and thermometer high,

augur complicated fevers. In the first case the north, in the second the south, will be the prevailing winds. The former will be most dreaded by the person with weak chest—the latter will be feared by the invalid from remittent and intermittent fevers.

A vast region is occasionally so divided as to present these two exposures and corresponding climates, and physical and moral peculiarities and diseases of the people. Thus, Asia is divided by the Himalaya range of mountains, into northern and southern. The one exhibits its many bleak steppes and vast plains of Thibet and Tartary, inclining towards the river Oby and the Frozen Ocean, and constantly exposed to the piercing northern blast. The other, opening south into the fertile valleys of Hindostan, exhibits a gradual decline, ending at the Indian Ocean. What is here seen on a grand scale, and marked by corresponding magnitude of effects, may, in a minor degree, be pointed out in every country, where a range of hills extending east and west, have a gradual declination to the north on one side, and to the south on the other. Montpellier, so long and so unmeritedly praised as a fit residence for the consumptive, presents this peculiarity, and so well aware are the physicians and inhabitants of the difference in exposures, that it is on the southern side alone, that the invalid is permitted to promenade; and when the health is in danger, a house is selected sheltered from the keen northern blast.

The *mistral*, or north-west wind, and the *beeze*, or north-east, are those from which persons suffering with weak chests, and liable to phthisis, desire most to be protected, and the promised exemption in this particular, is what induces so many English and other foreigners to resort to Nice, Villa Franca, and Hieres, on the coast of Provence. Not more certainly will they be attacked by intermittent fevers, who are exposed to a southern wind, sweeping over low and marshy grounds, than those, constituted as above mentioned, will sink under phthisis pulmonalis, by inhaling a bleak northerly wind sweeping from the distant hills and mountains. This is well illustrated in the climate of Hieres near Toulon. The ground between the

town and the sea to the south-east, is mostly marshy, and not unfrequently gives rise to remitting fevers among the inhabitants during the summer; while that part of it exposed to the north-west, especially in winter, renders the residents liable to pulmonary consumption. If a miasm, borne on the south-east wind, be the cause of fever in the summer, why ought we not to invoke the necessity of some deleterious principle, or miasm conducted by the north-west wind, as the cause of phthisis. But if the sensible qualities of the air be admitted as adequate to produce or keep up one disease, I cannot see why they may not be equally operative on the other. When HUXHAM tells us he has seen an epidemical catarrhal fever assume the nature of a pleuro-peripneumonia in bleak and elevated situations, whilst in lower ones it approached nearly to the nature of a slow, or so called *nervous* fever,\* he very explicitly points out the influence of localities and exposures, without the unphilosophical intervention of miasma, which the modern school would conceive necessary to explain the difference of the diseases above mentioned. He very properly adds, that the reason is hence seen, why, in the same reigning disease, a different method of treatment is lauded in different places. A variable season will, on this principle, by representing as it were various situations, give us both phlegmasiæ of the thoracic cavity and intermittent fevers. Sometimes, says the author already cited, a quotidian, a semi-tertian and tertian fever, will prevail epidemically at one and the same time, with pleurisy and peripneumony—as happened in the early part, (March, April, and May,) of the year 1744. Authorities to this effect might be multiplied without end, but the reader, desirous of a brief, lucid, and impartial history of all the circumstances in situation, as regards exposure to particular winds, and their succession and alternation, as well as the influence of the seasons, ought to consult the great father of our art, the *principium et fons*, in

\* Quid quod et interdum vidi febrem *catarrhalem epidemicam*, *pleuro-peripneumonix* naturam induisse per loca montosa et algida, dum in humilioribus ad lentam proximè, (seu nervosum ut dicitur,) febrem accessit. Prefat. p. 185-6. Opera Physico-Medica.

his works, *De Aqua, Aere et Locis*; *De Diæta*, and *De Epidemiis*, and also his *Aphorisms*.

Noxious as are, however, the effects of certain winds and their alternations, we have still more to fear from a stagnant atmosphere. "It is probable too, says Ferguson, that the healthiness of seasons in unhealthy climates depends less on the *amount* of heat and moisture than on the ventilation of the climate by powerful regular trade winds, like the trade winds between the tropics; for whenever these have been withheld for a time, the accumulated morbid emanations from underground moisture will act upon the human body like the accumulated typhoid principles in crowded hospitals, when undiluted with a due proportion of atmospheric air." The pernicious consequences of a dry summer in the West Indies, are perhaps, mainly referable to the accompanying stationary atmosphere. The fact itself is so generally admitted as to require no additional enforcement here, but the explanation of Dr. Ferguson is manifestly speculative. Blind to, or wilfully overlooking the evident condition of the air, physicians constantly seek for some unknown, unappreciable, and we may add, monstrous principle, which, as it has no fixed origin, cannot be subjected to rigorous laws, but may be pressed into the support of every hypothesis. When those unhappy creatures, pent up during a night in the Black Hole at Calcutta, perished in such numbers before morning, was it by the effluvia from under ground moisture, or by any effluvia from the ground whatever? When the hatches of a ship are shut down for a few days in bad weather, and there are many people breathing between decks, is it from such effluvia or miasmata that the consequent typhus originates? These may, perhaps, be called extreme cases. What then shall we say of *cholera infantum*, so fatal to children in the hot, stagnant atmosphere of a city, and in parts of it where no miasma can be or is suspected? That it proceeds from the irritations of close heat, teething, and crude or improper ingesta. When this disease prevails, as it occasionally does in parts of the country, as in the Miami district of Ohio, according to Dr. DRAKE, is it then the result of miasm from marshes, or effluvia from underground moisture?

Thirty or forty children have died in a week in this city from cholera, who have exhibited by the symptoms in life, and appearances after death, extensive structural alteration in the stomach, intestines, and brain; yet in their case we hear little if any thing of miasma. But let two or three adults die in the lower part of the city of yellow fever, and the changes are rung upon the words importation and contagion—or miasm. I do not well see by what scale of moral law we can feel less sorrow or alarm at the death of thirty children, some of them their parents sole hope of present peace and after joy, than at the death of three or four drunkards; or by what philosophy we admit evident natural agents and processes, as sufficient causes of disease in the former, and require an unknown, unappreciable additional cause in the malady of the latter.

A very brief study of the influence of temperature on the human frame will soon convince us, that heat in summer and cold in winter, are respectively the predisposing or exciting causes, (according to the condition of the individual at the time,) of the epidemics of those seasons. If in spring and autumn diseases are more numerous and complicated, the reason is to be sought in the extreme states of the atmosphere, as heat and cold, moisture and dryness, northerly and southerly winds rapidly alternating with each other. The noon day sun is that of summer—the midnight cold is that of winter. The climates called temperate, which are so noted for the vicissitudes of weather, have, from this cause been the greatest sufferers by epidemics. “The great sickness, says Pringle, in his work on the Diseases of the Army, commonly begins about the middle or end of August, whilst the days are still hot, but the nights cool and damp, with fogs and dews; then, if not sooner, the dysentery prevails, and though its violence abates by the beginning of October, yet the remitting fever gaining ground, continues throughout the rest of the campaign and never entirely ceases, even in quarters, till the frosts begin.” p. 104. HOME, in his Dissertation on the Remittent Fever of Flanders, makes similar observations on the extremes of temperatures, and of dryness and moisture. The subsidence of the more simple, though often

violent diseases of summer, is but the prelude to those more complex ones of autumn. "About the time when the tertians begin, the cholera morbus, rash, and essere become frequent and epidemical in a less degree, but are seldom met with after September, while the tertians continue till winter."\* Corroborating what had been advanced by this writer under the same head of the Rise Declension, &c. of Epidemical Diseases for the year 1744, viz. "As the summer and autumnal weather of one year never varies much from that of another, so the same tribes of distempers return regularly with the seasons, and succeed each other in the following order."

In Malta, in 1813, the difference of temperature between day and night was sometimes from fifteen to eighteen degrees, and was the greatest during the most fatal period of the pestilence; and we learn from respectable authority, (Dr. WYTHE,) that plague has been ever most prevalent where the difference between noon and midnight temperature has been greatest, and at the seasons of sudden change from dry to moist, and from warm to cold. In no country is there, at some seasons, a greater difference between the temperature of day and night than in Egypt, where the days, at times insufferably hot, are succeeded by nights so extremely cold, that three, sometimes four blankets, are required by a person to keep himself warm.

Night, we are told by the miasmatisers, is the time when there is the greatest precipitation of marsh effluvia, and consequently the period during which residents in sickly countries are in the greatest danger of being attacked by fever. The fact of danger during the night is unquestionable, the miasmatic explanation purely speculative. "At the hottest time of the day, the air of the marshes is clear, serene, without smell, it may be breathed without danger; *it is, however, then that the exhalation of marshy effluvia goes on with the greatest activity.*"† How is this? At the very time when the pestiferous effluvia are given out with the most activity we can then inhale them with the greatest security!

\* Cleghorn, Diseases of Minorca.

† Montfalcon, p. 89, 90.

The explanation furnished of this paradox, is that the miasmata are volatilized with the aqueous vapour in which they are suspended. But moisture is not required, according to Dr. Ferguson and others, for the evolution or suspension of the malaria. We will, however, suppose it thus volatilized, and harmless during the day, when it is given out in the largest quantity. At night it descends, we are told, with the dews, or according to others it descends from the atmosphere and ascends from the ground at the same time. The noxious emanations are then approximated, says LANCISI, in consequence of the diminished temperature, and act of course on the persons exposed to them with increased energy. Let us ask, whether this condensation or approximation of miasmatic particles makes them more powerful, now that the supply from the earth is much less than it was during the day; and whether sublimation and greater evolution ought not to be equivalent to condensation and minor evolution.

An inquiry into the actual and perceptible atmospherical changes after the decline of the sun below the horizon, will show how far they will go to explain the attacks of disease during the night.

At variance with pre-conceived opinions, is the now well established fact, that dew is only in a small proportion the product of vapour rising from the earth at night, but is rather the result of the precipitation of moisture from the air, on its meeting the colder substances on the surface of the earth, or within a few feet of it. This coldness depends on the radiation of heat by the earth during the night in return for the heat radiated to it by the sun during the day, and which, if suffered to accumulate, would destroy the present constitution of our globe. The surface of the earth thus rendered colder than the neighbouring air, condenses a part of the watery vapour of the atmosphere into dew. This condensation is, of course most perceptible on those bodies which soonest become cold by the radiation of their heat. For this reason we find that the quantity of dew on grass is much greater than on gravel-walk or garden mould, the former being often ten or twelve degrees of Fahrenheit colder than the latter. *The*

*fluid then of dew appears chiefly where it is most wanted, on herbage and low plants, avoiding in a great measure, rocks, bare earth, and considerable masses of water.* Bright metals exposed to a clear sky in a calm night will be less dewed on their upper surface than other solid bodies, since they lose the smallest quantity of heat by radiation to the heavens.

In corroboration of this theory of dew, we learn that in Egypt a little before the rising of the Nile, and consequently when the ground there is in its driest state, dew becomes exceedingly plentiful, though little or none had formed before, when the earth was somewhat less dry. A parcel of wool on a board, raised a few feet from the ground, acquires more dew than wool upon a grass-plot. Lastly, if dew were the vapours from the earth it should never appear in any considerable quantity without being accompanied with fog or mist; now it has been found that the formation of the most abundant dew is consistent with a pellucid state of the atmosphere; and in Egypt, according to HASSELQUIST, during the season remarkable for the most profuse dews, the nights are as resplendent with stars, in the midst of summer, as the lightest and clearest winter nights in the north.

From these facts we learn how the inhabitants of certain hot countries, who sleep at night on the tops of their houses, are cooled during this exposure, by the radiation of their heat to the sky. We also explain the greater chill which we often experience upon passing at night from the cover of a house into the open air, than might have been expected from the cold of the external atmosphere. On a clear night in an open part of the country, nothing, almost, can be returned from above in place of the heat which we radiate upwards. In towns, however, some compensation will be afforded, even on the clearest nights, for the heat which we lose in the open air, by that which is radiated to us from the surrounding buildings.

*Bodies may be protected from dew by a canopy of matting or sail-cloth, even though the sides be entirely open, and a free passage of air admitted under it. Any kind*

*of cloth, even placed perpendicularly to the earth will afford protection from dew, by preventing, in part, the loss of heat by radiation.*

Though dew, conformable to the principles here laid down, can never be formed in temperate climates upon the naked parts of a living and healthy human body during night, yet in very hot countries its uncovered parts may sometimes, from being considerably colder than the air, condense the watery vapour of the atmosphere, and hence be covered with a real dew, even in the day time.\*

We may from the above, account for most of the noxious effects of exposure to night air. The fatigues of the day place the body in a state peculiarly liable to be affected by any unusual impression, whether of atmospherical vicissitudes, or of ingesta, or the passions. In the case of persons sleeping exposed to the night air, we have a reduction of temperature of many degrees; continued loss of heat by radiation from their bodies, and the sudden application of humidity to a skin strongly excited by the sun during the day, and partially exhausted in function by prior profuse perspiration. The conditions for a great formation of dew and precipitation of moisture will be the prior evaporation from masses of water, and the adjoining earth in such a state as to radiate freely its caloric, and be much colder than the superincumbent air. Now, we know, that both these conditions are united in marshy districts, and, of course, in them have we to apprehend the worst effects of humidity. But dew is not, as we have seen, the product of air in the neighbourhood of marshes alone. It will fall heavy where vegetation is abundant, and may be seen on hills as well as on low grounds. Exposure to it and the colder night air, will in the spring produce pleurisies and rheumatisms; in the summer cholera and diarrhœa; in the autumn dysentery and periodical fevers. Disease will display itself according to the order of parts which were rendered susceptible by the pre-existing season or atmospherical constitution. Thus, ophthalmia will be com-

\* For further particulars the reader is referred to Wells on Dew.

mon in Syria and Egypt, with those who sleep on the house tops exposed to the night air and dews. The great heat and light of the day, increased by the reflection from the sand and rocks, had made in those persons the eyes their most irritable part, and, of course, that which will first suffer by any succeeding agent suddenly applied to the body. Soldiers on a campaign, and similarly exposed to night air, after a long and fatiguing march, will have some dysentery, others fever, according to the previous state of their intestinal canal, arising out of individual peculiarities, or the use of crude ingesta.

In low marshy districts, impure water and solid food of an imperfectly nutritive nature, are to the full as contributing causes of fever and disease as the alleged miasm.\* In fact, if we attend to diet, clothing, and protection from certain winds and atmospherical vicissitudes, this last, so much dreaded agent, is reduced to a cypher, a mere negation. The person who would wish to escape periodical fever, need not suffer his mind to be haunted with the idea of a pervading poison, but may confidently abide by those rules and precautions which he would adopt if he wished to escape any epidemical disease.

If the air of marshy countries were not merely a cause among others of sickness, but the cause, as the miasmatisists teach us, is it not inconceivable, that there should be so great a difference in the inhabitants of such districts as regards their exemption from fevers when all are necessarily obliged to inhale it? And yet we know, that in Zealand itself, "such as live well, drink wine, have warm clothing, and good lodgings, do not suffer so much during the sickly season as the poor people."† I have myself been told by a French officer, who served in that country, that, if common prudence were used, and wholesome drink taken, he and his companions had little fear of fever.

The capital error of the miasmatisists, whether of the Culenian school, or the associates of Bancroft and of Armstrong, consists in predicating on any particular cause, the course and

\* See Hippocrates *de Aqua*, &c.

† Doctor Wind, quoted by Lind.

symptoms of a malady. It has been strangely forgotten, that various causes, insolation, a blow on the head, a sudden gust of passion, for example, will produce frequently similar effects; that the stomach and digestive system may be disordered by a general impression on the skin, as in the sudden obstruction of perspiration; by a chronic irritation of this same part; by irritation of other parts, not less than by direct irritation of the stomach itself. Nothing could be more fallacious than the reasoning of Armstrong, that, because in typhus there was in degree the same diseased alteration of the mucous surfaces of the intestinal canal and pulmonary apparatus, and of the brain, as in remittent fever, that therefore typhus was, like this latter, the product of miasm.

The disease called softening of the brain, and which acknowledges a variety of causes, not unfrequently exhibits towards its close, all the symptoms of typhus fever. The thirst is commonly increased; appetite lost; lips and teeth dry; tongue rugous and chapt, at first red and afterwards brownish or blackish. Deglutition is often difficult, almost impracticable; the efforts at the act sometimes convulsive. In some cases copious vomiting, first of food, subsequently of bile; frequently great abdominal tenderness, sometimes involuntary discharge of fæces, more frequently constipation; unconscious escape of urine; respiration often tight. The pulse is in some instances accelerated and stronger than ordinary.\* Could the respective advocates of miasm and contagion in typhus draw a stronger picture than the above, to enforce their opinion of a poison having been applied to or taken into the body.

In conclusion, I think we are justifiable in the present stage of this inquiry, in denying the existence of any such separate, peculiar, or specific morbid cause, as miasm or malaria. The attempts to explain its origin and production are so far entirely unsuccessful, and involve those who persevere in them in a tissue of contradictions, which would not be advanced or received by any known laws of evidence.

\* See the works of Lallemand and Rostan, “*Sur le Ramollissement du Cerveau.*”

The argument in favour of miasm, drawn from the periodical character of fevers, which are its alleged product, has no better foundation in fact, since periodicity is common in degree to all diseases, and in a marked manner to many which were never thought of miasmatic origin.\*

The seasons when intermittent and remittent fevers most prevail, is that in which all the evident causes of disease most abound, and when many are exasperated or return, which acknowledge no paludal origin.

The recurrence of intermittent fever in the spring, in those who have suffered from it in the preceding autumn, cannot be accounted for by any miasmatic theory, and is after all no more surprizing than the recurrence of gout, hæmoptysis, and rheumatism.

That a residence in marshy countries subjects to intermittent fever, is an undeniable fact—and that they who live on broken hilly districts are liable to pulmonary diseases, is also unquestionable. If locality explain the latter, it may equally explain the former, without recurrence to imaginary agencies. In the same county of Lincoln, in England, the inhabitants of the fens are sufferers from intermittent fevers; those of the wolds or hills, are obnoxious to catarrhs, pleurisies, and phthisis. If an exchange be made of habitation in these two cases, there will be an exchange of diseases. Why then demand miasm as a cause of the fever, and refuse it as a cause of the pulmonary disorders?

The winter season, by placing the inhabitants of the low grounds under the influence of its atmospherical constitution, will cause catarrhs, pleurisy, &c. The autumn, if very wet, may make the tenants of the hills and mountains liable to intermittent fevers. In season and situation then, as subjecting a country to atmospherical extremes and vicissitudes, and in impure drink and crude indigestible food, we may find the causes of epidemical diseases, as has been done by Hippocrates, Sydenham, Huxham, and Cleghorn, to the rejection of the misleading unit cause, miasm.

\* See my Essay on Periodicity and Lunar Influence in the last number of this Journal.

[I received the following letter from Dr. Holcombe, very soon after it was written; it was accompanied with a request, that I would, in some way or other, give my opinion upon its several subjects. I found it so interesting, and so practically important, as to determine me to give it publicity in this number of the Journal. In doing this, I hope I shall not be considered as manifesting an overweening self-love, by not expunging the kind, or rather flattering, mention made of myself. I would *most gladly* have done this, had it been practicable, without injury to the other parts of the letter.

WILLIAM P. DEWEES.]

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ART. VIII. *Letter from Dr. GEORGE HOLCOMBE, of Allentown, N. J. to W. P. Dewees, M. D.*

Allentown, N. J. Nov. 21, 1825.

DEAR SIR—I have just finished reading your System of Midwifery, and have to request you to accept my thanks, in common with the profession, for the much novel and valuable information which it contains.

Permit me to call your attention, for a few moments, to several subjects, which have been but briefly noticed in your work. The first is Ergot. This extraordinary agent owes its introduction into the Materia Medica entirely to American physicians; and, as yours is the first System of Midwifery which has issued from an American source, since its use has become general, the profession, both here and abroad, looked to your pages for a more particular account of the properties and uses of ergot than has heretofore been given. I am afraid they will be much disappointed, as I frankly declare to you I have been, in finding it passed by almost unnoticed. Much, it is true, has already been written concerning it; and, I am aware, that practitioners are supposed to be well informed as to its properties, and the cautions necessary to be observed in its administration. But this I am persuaded is a mistake—and, in my opinion, you could not more effi-

ciently subserve the cause of humanity and obstetrics, than by devoting a chapter, in the second edition of your work, to the consideration of the uses and abuses of this article.\*

The scruple dose, as a general prescription for aiding the expulsive stage of labour, I am convinced, from considerable experience, is exceptionable and dangerous. In ordinary cases, no mischief, it is acknowledged, will result from the exhibition of so large a dose. But the child, if it be unusually large, or the pelvis faulty, or if the accoucheur have to contend with a first labour, will always be put to imminent danger, and frequently destroyed by scruple doses, when its safe delivery might have been effected, either by the unassisted energies of the mother, or by small divisions of the dose, frequently repeated.

Your zeal in proscribing the use of the crotchet, will doubtless obtain, as it certainly merits the plaudits of your brethren—at least of the more enlightened portion of them. But if the use of the terrible instrument just named be as reprehensible, (and who will presume to deny it?) as you have represented it, how much less so, or rather how much more so, is the intemperate use of an agent which sacrifices a four-fold, if not a twenty-fold, greater number of victims? More children, I am satisfied from what I have seen and heard, have already perished by the injudicious use of ergot, during the few years which have followed its introduction into the practice of this country, than have been sacrificed by the unwarrantable use of the crotchet for a century past! This, if correct, is a most serious fact; and of its truth I do not entertain the slightest doubt.†

\* In my "Essay on the means of lessening pain," &c. I have given the history of the "use of the Ergot;" I did not think it necessary to repeat what had already been said; especially as we are constantly receiving some practical hint, besides regular histories of this article through the medium of the different Journals published in this country.

† I rarely employ the "ergot" in any dose; and were I to establish a proportion between the cases in which I do employ it and those in which I do not, I would at a venture say, not more than one in seventy-five or eighty cases. When I do use it, it is in scruple doses—that is, I give this

But notwithstanding the strong language I have just used, I regard ergot, when cautiously administered, as a most interesting and valuable adjuvant in the practice of midwifery. I have used it constantly, since Dr. STEARNS first called the attention of physicians to it. For some time I used the scruple doses, or corresponding doses of the decoction, which I am afraid, are every where yet too common, but soon abandoned this practice in consequence of several fatal demonstrations of its impropriety. Since then I have administered it in very small doses—say from two to three grains, which I

quantity, and if it fail to produce its full effect in fifteen or twenty minutes, I give a second dose, should this fail I urge it no further.

I never exhibit this remedy, but after the rupture of the membranes; the full dilatation of the os uteri, and a disposition in the external parts to relax. When given under such restrictions, I am disposed to believe no injury can arise from its use, at least I have never seen an instance of a still birth, which could be attributed to the action of this medicine. This result comports with the experience of the friends I have consulted upon this subject; among those I may mention Drs. James and Chapman.

I must not, however, be understood to deny, that mischief has arisen from the use of this drug: for, I believe, much has followed its incautious use. I am disposed, nevertheless, to think, that the evils complained of from this cause, have arisen from its having been administered at improper times, rather than from improper quantities. This belief is founded upon the certain knowledge of its having been used, before the conditions proper for its employment were present. I have known it given before the rupture of the membranes, the dilatation of the os uteri, or a disposition in the perinæum to relax.

It is noways difficult to account for still-born children, under such circumstances, especially where a rigid perinæum is to be contended with, as but too frequently happens with first labours. For the action of the “ergot” is very decidedly such, as to produce a most powerful state of tonic contraction of the uterus, as well as to augment the alternate.

In consequence of this, the placenta becomes strongly compressed between the child and the uterine parietes; its vessels will, therefore, be more or less obliterated, and the circulation between the mother and child so retarded, as to interrupt the due oxygenation of the blood: the child will of course die, because it receives little else than black blood.

I think it an error, to suppose that the “ergot” has any direct injurious agency upon the child; when it does mischief, it is by an indirect action.

repeat as often as may be necessary: and in this manner I am *generally* able to effect my object; and *always* without injury to the mother or child. In fact, I have never known a child's perishing from ergot, administered in this cautious manner.\* I prescribe it now unhesitatingly in first labours, and even in cases of contracted pelvis: and such is the extraordinary energy imparted to the uterus, by the slow but persevering mode of exhibition which I have adopted, that I have very rarely found it necessary, for several years past, to resort to the use of the forceps.

Independently of the power of ergot in aiding the expulsion of the child, it seems to possess other important properties in the practice of midwifery. I have never seen a case

\* It is highly useful to be informed, that small doses of the ergot, are equally efficacious with large, as regard the main object of its exhibition; and the more especially, as Dr. Holcombe assures us he has never known either the mother or child to sustain any injury from this mode of employing it. Of the efficacy of this plan I can say nothing from my own experience; though resolved to put it in practice in the first case in which it may be proper to employ it. But the character of Dr. Holcombe is every way sufficient, in my mind, for the profession to place the fullest confidence in his experience.

It would seem, however, that much must depend upon the condition of the uterus and perinæum at the time of its exhibition; since, the same results have followed the use of the larger as of the smaller doses, agreeably to the experience of Drs. James, Chapman, and myself. Now, as it is certain, that in the practice of the gentlemen just named, the "ergot" was not given but under the proper conditions for its exhibition, it seems to follow, that much more is attributable to the time at which it is exhibited, than to the quantity given.

Dr. Holcombe assures us, that he gives doses of but "two or three grains, and repeats them as often as may be necessary." I regret he has not been a little more explicit upon this point, by informing us at what intervals, or what constitutes the necessity for repetition. I should, however, conclude, that he has been governed in the use of his small doses, by the same rules, as influence other practitioners in the employment of the larger doses, namely, a failure in efficacy; and the interval may be every fifteen or twenty minutes.

It may be oftentimes of great practical value to know, that by repeating small doses of the "ergot," we may gradually urge the uterus to efficient action; for sometimes, without doubt, it is urged beyond the necessary degree.

of puerperal fever follow its administration. This fact may be accidental, and confined to my experience; but the remark I have thought might be interesting to you as a teacher of midwifery—at least worth noticing.\* It sometimes completely extinguishes the lochia; but this effect has never resulted, as far as I have observed, in injury to the mother.† I prescribe it, which I believe is a common practice, when flooding after delivery is apprehended. Also in cases of partial prolapsus of the uterus, and of habitual bearing-down, from whatever cause the last distressing affection may arise; and I am much deceived, if great comfort is not frequently derived from this precautionary exhibition of the medicine, in each of the diseases just specified.‡ But I am afraid I am extending my observations beyond your time and patience; I will therefore close them, by repeating the suggestion which I have already taken the liberty to make—that you would confer a great benefit upon midwifery, by devoting a chapter in the second edition of your work, to the medicinal history

\* It would be a most valuable acquisition to our remedial means, did the “ergot” contribute to the lessening of the cases of puerperal fever. Upon this point, I can say nothing practically; it may be true, as stated by Dr. Holcombe, that this may be confined to his own experience, or rather that it may be merely coincidence; yet it deserves most serious attention, for this substance may have a prophylactic power. I would, therefore, earnestly recommend this subject to the attention of the accoucheurs of our country; and especially to those who may inhabit districts in which this too fatal disease becomes epidemic.

† “It sometimes completely extinguishes the lochia.”<sup>a</sup> I have never seen this effect follow the use of the “ergot.” Nor do I believe it generally necessary that it should do so; for if the labour has been well conducted through all its stages, the lochia will rarely be too abundant. See “System of Midwifery,” Art. Lochia. Yet it may be highly important to be in possession of a remedy when the diminution of the lochia may be an object.

‡ The usefulness of the “ergot” in the last specified affection, namely, “habitual bearing-down,” is certainly contrary to all reasoning upon the subject; and is one of the many instances, in which we are obliged to make speculations yield to experience.

<sup>a</sup> I presume Dr. H. only means by “extinguishing the lochia,” a great diminution of it.

of ergot. The profession, I repeat, expect it of you; and, permit me to add, from the situation which you occupy in American obstetrics, has a right to expect it of you.

Regimen is another subject but briefly noticed in your work. In the short chapter which you devote to it, you merely observe, experience has proved, that lessening the quantity of food on the part of the mother, has no influence upon the size of the child—or words to this effect. My attention has been turned, for some years past, with much interest to this subject; and I have been led to believe, from the result of several cases, that, by means of a medicinal regimen, the size of the fœtus in utero, may be very sensibly lessened, without jeoparding its safety, or seriously impairing the health of the mother: so much so, that a child, which would otherwise weigh at birth ten pounds, may be made to weigh five pounds! My opinion is founded upon the following facts: in June, 1817, I delivered with the crotchet, (the forceps and ergot had failed,) after a fearful labour of sixty hours, a lady with her first child. She was thirty-two years of age, and had a pelvis more deformed than any I have ever met with. In fact, it is the only pelvis, actually deformed, which, in a practice of sixteen years, I have had to contend with. This lady soon conceived again. Her child, however, perished about the seventh month, but was not expelled until the eighth; and then, notwithstanding it was in a state of putrefaction, the labour was protracted, severe and difficult. I ought to mention here that the first child weighed ten pounds and a half, and the second, had it lived, would to all appearance have attained the same size. Mrs. — soon found herself pregnant again, but almost immediately afterwards, began to complain of symptoms of general dropsy, which required for their suppression the daily use of medicines, during the whole period of gestation—particularly a pill composed of calomel, squills, and digitalis. The calomel kept her system almost entirely under the mercurial influence; and the squills and digitalis assisted in nauseating her stomach from day to day. At the close of a full period, she was delivered of a healthy child, weighing four pounds and

a half, after a very sharp labour of five hours ! The remarkable difference between this child and the two first, struck me forcibly; and I was disposed to account for it, by referring it, in a great measure, to the medicinal treatment to which the mother had been subjected; and I was determined to test the accuracy of the conjecture, by direct experiment, as soon as a proper case should come within my control. One soon occurred. A pregnant woman, who had never borne a living child, but lost three, two of them by instrumental delivery, was easily prevailed upon to submit to a course of medicines, by the prospect held out to her of an easy labour and a living child. She commenced her course about the fourth month of gestation, and continued it very perseveringly until delivery, which was effected without difficulty. The child, (which was alive and healthy,) I did not see, being absent from the neighbourhood, but was assured that it was rather smaller than children usually are, weighing perhaps about five and a half or six pounds. Her other children had weighed from ten to twelve pounds. One of them I saw and weighed myself. It rather exceeded eleven pounds. The effect of treatment, in this case, I considered decisive, and I was confirmed in this opinion by this woman's next child, which weighed eleven pounds, and was expelled by means of ergot, after a most painful and difficult labour of forty hours. The woman had refused, during gestation, to submit to treatment, or rather her husband, (having been ridiculed by his associates on account of his dwarf—a plump, healthy little thing, by the way, as any in the neighbourhood,) had compelled her to refuse. Since this case, several other women have submitted to medicinal regimen, and the results has been uniformly the same: the child in every case being considerably reduced in size, but born alive, and to all appearances, with unimpaired health and constitution. Thus, sir, have five women, (four of which had never borne a living child,) been delivered of living children, with comparative ease, by means of a medicinal regimen. In four of the cases the women had lost, (whether necessarily or not, I cannot say,) by the crotchet and other means, eleven children. In the fifth case,

the labour preceding the one which I have noticed was frightfully severe, in consequence of the extraordinary weight of the child—fourteen pounds! Delivery was effected with great difficulty by embryotomy. In the next labour, (the one treated,) the child weighed but four pounds! and although a breech presentation, was expelled in a few pains. I intend to put this question at rest, if opportunities offer, by further experiments. Permit me, however, to entertain in the meantime, with great confidence, the following opinion: that women with faulty pelves, may be enabled by means of a rigid course of medicinal regimen, to bear children alive, who could not by any other known method.

To illustrate more fully my practice, I will detail to you the treatment of a single case.

June 1st, 1819.—Mrs. ——— aged twenty-four years, is pregnant with her fourth child—has lost three children—owing, I was told, to their uncommon size and her contracted pelvis—is supposed to be about four months advanced in gestation—is willing to submit to any kind of treatment, however severe, to procure an easy labour, and be the mother of a living child. Let her take a pill of the following prescription, morning, noon and night—take also, twice a week, forty drops of laudanum, and be bled every month.

R. Squills, 48 grs.—Calomel, 18 grs.—Digitalis, 6 grs.—Make twenty-four pills.

August 1st.—Has taken the pills, with a few short intermissions, very regularly—mouth slightly sore—appetite impaired—feels feeble and dejected, but is willing to persevere in the treatment. Has taken the laudanum and been bled twice.

Let her take three grains of squills, night and morning—laudanum three times a week, and continue the bleeding. As soon as the mouth is well, resume the calomel and digitalis.

October 1st.—Resumed the calomel and digitalis. August 10th—and has continued the calomel, with short intervals of omission, constantly since—has omitted the digitalis every other week. Continue the treatment; increasing the squills as

far as the stomach will bear—omit the digitalis. Take fifty drops of laudanum every other day.

November 17th. Delivered yesterday, after a labour of six hours, of a plump, healthy-looking child, weighing five and a half pounds. Her other children had averaged ten pounds!

Had I the management of a case of more than usual deformity of pelvis, I would commence the treatment as soon as conception was fully ascertained, and put the patient under a full course of various medicines, particularly of opiates and mercurials, which I would urge throughout gestation, as far as a prudent regard for the mother and child would permit.

It would be very gratifying to me, if you would give the regimen which I have suggested, a fair trial in your practice, whenever a proper case shall offer. Cases of deformed pelvis are seldom found in the country. Our resort to the use of the crotchet is constantly owing to the increased size of the child, rather than a want of capacity in the pelvis.

The principle upon which my practice is founded, is briefly this: to derange digestion, and keep the liver, that great laboratory of nutrition, in a constant state of morbid excitement. In confirmation of my theory, I have collected a number of cases of very small children, following bilious and other fevers, in which much medicine, particularly mercury had been used. You have frequently noticed, I presume, the same fact. Habitual opium-takers also bear small children. There is a remarkable case of this kind, at this moment travelling the round of the newspapers, copied from some of the British Journals.\*

\* The whole that Dr. Holcombe has urged upon the subject of regimen and medicine, to diminish the size of the fœtus in utero, is well deserving of attention, especially in Europe, where the necessity is much more frequent, as well as much greater, than in this country. The combination of medicine and regimen may overcome difficulties that would not yield to either alone. Yet I cannot but feel strong doubts of their united powers in the cases under consideration. Certain it is, I have seen strong and robust children born from consumptive parents, and this even in the last stages of it, where the emaciation was great, and the debility extreme. I never met with but one decided exception to this; and this was in the case related in my "Essay upon Retrover-

Whenever your leisure will permit, I shall be happy to hear your opinions, if ever so briefly given, on the several interesting subjects to which I have taken the liberty of calling your attention.

I am, dear Sir, with great respect, your obedient servant,  
 GEORGE HOLCOMBE.

PROFESSOR DEWEES.

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ART. IX. *Remarks on the Bilious Fever of Virginia.* By  
 ALEXANDER T. B. MERRITT, M. D. of Hicks' Ford, Va.

THE tract of country in which I live, borders on our sister state of North Carolina, is situated nearly midway between the Atlantic coast and the mountains, and distant about twenty miles above the head of tide water. It is gently undulating and agreeably uneven, though near the rivers which pervade and wash it, and on its western precinct, it swells into more considerable eminences, and may be considered hilly and broken. A large proportion of the land has never been reclaimed, consisting of flats, bottoms, marshes, and swamps on small water courses. These and a great many ponds of confined and stagnant water, necessary for the supply of numerous mills, often cover extensive surfaces, which are denuded by the desertion or the evaporation of the water, leaving their slimy banks and beds spread over with the most noxious vegetable accumulations. The vegetation is rapid and luxuriant; and particularly for a few years, since the practical adoption of the enclosing and non-grazing system; there has been an abundant mass of vegetable matter left on our fields for gradual decomposition and decay, the prolific sources of morbid emanations.

The inhabitants are generally of that class, who possess a  
 sion of the Uterus:" and certain it is, I have seen healthy and large children from mothers who had been profusely salivated; yet the union of abstemiousness with purging and salivation, may effect the desirable reduction of the fœtus.

“golden mediocrity” in their circumstances, and though they live well, seldom indulge to a criminal excess in those luxuries which wealth alone can procure for a vitiated appetite. They cannot be considered as very temperate when compared with other people. Frequent instances of habitual drunkenness occur. Many persons too, quit the pursuits of industry for the gratification of their pleasure, as they depend on the labour of slaves, and not on their own exertions, for the support of their families or the acquisition of property. This unhappy part of our population are generally treated with humanity, are well clothed, have an ample provision of nutritious, though somewhat coarse food allowed them, and live in small, but generally comfortable cabins. Their labour is light, and such as they can easily bear.

The climate is variable and uncertain, subject to violent and sudden changes. One day breathes the mild and balmy breezes of the spring, and the next saddens with the cold and cheerless gloom of winter. But allowing for these great atmospheric vicissitudes and sudden transitions, the summer is long, dry, and hot: the winter cold for the latitude: and the spring and autumn form intermediate stages between the extremes, are very changeable, and partake of heat and cold.

From the general nature of the climate of this section of the state, and the topographical delineation which I have offered, it will be obvious to all enlightened physicians, who have directed their inquiries and bestowed their attention on the ætiology of diseases, with the care and diligence which the importance of the subject demands, that our summer and autumnal epidemics must belong to the “bilious family.” Being fully persuaded that much of the clearness and precision with which diseases are described in our modern systems of physic, is the result of theoretic refinement, and metaphysical abstraction, I shall, in a great measure, discard the fastidious distinctions, and fanciful differences which have been so ingeniously and so industriously traced, and endeavour in the following remarks, to pourtray the different forms of the fever which is the subject of this essay, as they are really presented to the observation of the clinical

examiner, in the great and correct system of nature. The remote causes of fevers, in which all genuine differences are laid out and radicated, are of a nature so extremely subtle, as to have eluded the examination of those who have attempted to analyze and explain them. But notwithstanding they are thus closely concealed from our view, their influence is, we are compelled to say, but too well known, "*causa latet, vis est notissima.*" Fevers, however, by whatsoever cause produced, exhibit phenomena so essentially and so extensively analogous, that principles, whether therapeutic or pathological, applicable to one, will, by a little modification, extend to all. Their causes, which may at first view appear opposite and unlike, will, when we come to contemplate their influence as stimulants, be forced to walk in narrow and circumscribed limits, and from a similitude of operation, their great and important effects on the vital organs will be nearly the same. Proofs and illustrations of the truth of this remark, both of a theoretical and practical nature, will be found in all febrile affections, whether they be produced by the specific impressions of contagious essences, by miasmatic exhalations, or by sudden and abrupt atmospheric vicissitudes.

This doctrine, so clear and comprehensive, having been ably unfolded and enforced by some late writers, I shall leave it, and pass on to the history of the bilious fever, which has prevailed among us for several years past, to a most alarming and desolating degree.

For the sake of order and accuracy, I shall arrange the different forms of the bilious fever, as it occurred to my observation, under three varieties: viz. the simple or excited, the inflammatory, and the congestive. I think these distinctions should be made for reasons of practical importance, as I am fully persuaded that they really exist in most febrile diseases, and will be noticed and recognised by all enlightened practitioners and pathologists.

The simple or excited variety of bilious fever being the least complicated, shall be concisely characterised in the first place. It has in reality a genuine febrile excitement, or a marked development of the hot stage, without any decided

marks of topical inflammation. This state of excitation is, however, preceded by one of oppression, and followed by exhaustion or the state of collapse. The most usual and obvious symptoms of the "forming state of fever," or the stage of oppression, are, a pallid aspect of the face, with an inattentive or dejected countenance; a dark or dull livid appearance of the integuments beneath, or surrounding the eyes; prostration of strength; a lessening of the general sensibility with a diminution of intellectual power; loss of appetite with nausea and even vomiting; a heavy and anxious sensation about the stomach; confined and hurried respiration, accompanied by sighing; whitish tongue, invested by a ropy or glutinous saliva; a heaviness, vertigo, and frequently an aching of the head; chilly sensations running over the surface and down the back, more or less permanent in the latter part, attended by pains extending to the loins, or sudden and frequent alternations of hot and cold fits; and a small, quick, weak and confined pulse, changeable as to frequency and force. These and some other symptoms prevailing, a distressing degree of lassitude and uneasiness is observed. Their duration is, however, various, but most commonly after having continued for a day or two, and in some cases longer, the nature of the disease is revealed by the supervening of an ague or a chill, a pathognomonic sign of bilious fever. A full and complete demonstration of the second stage now quickly ensues.

The height to which the febrile reaction now rises, is determined and graduated by the constitutional vigour or relaxation, and the weakness or energy of the impression of the miasmatic cause. The temperature of the body is considerably elevated; the pulse, which in the preceding stage was inelastic, now expands and becomes lively and resisting; the cheek is flushed, and the eye is dull and suffused; the breathing is quick, anxious, and imperfect; the lips are parched; the skin dry; the thirst great and insatiable; the tongue foul; and the mobility of the sensorium very evident. The brain is so much disturbed by the increased circulation through it, as to be affected by incoherence, and often by de-

lirium. The secretions are altered in their appearance; and the bowels become more or less torpid.

When this stage is pretty well established, it preserves a steady course for several days. There are, however, usually regular chills succeeded by rises of fever, which circumstance locates this modification in the remittent family. When the exacerbation takes place, the symptoms are then most urgent and distressing. The prostration is augmented, the tongue and skin become drier, and, in fine, all the secretions are diminished, except perhaps that carried on by the liver.

The stage of excitement having lasted for a time, according to its mildness or severity, from five to ten or twelve days, introduces that of collapse or exhaustion. This stage is announced by the disappearance of many of the symptoms of the preceding stage. If convalescence is to succeed, the pulse becomes softer, less frequent, and contracted in its volume; the tongue will be clearer and more moist; the skin relaxes and softens, with a reduction of its temperature; the thirst lessens in urgency; the breathing is deeper and more natural; a deposition of sediment in the urine can be discerned; the fecal discharges are improved in appearance; and slight spasms or crampish pains take place. If, on the other hand, a fatal termination is to ensue, all the above symptoms become aggravated and inveterate. The pulse becomes quicker, more frequent, and weaker; restlessness and jactitation are far greater; respiration is feeble and anxious; the tongue is darker and drier; the countenance is haggard and hippocratic; the voice is weak and altered; a peculiar cadaverous smell of the body takes place; and slight and troublesome convulsive fits of coughing follow. Hiccoughs, difficulty in swallowing, twitching of the tendons, coldness, visual illusions, deafness, and a low and muttering delirium, are the immediate precursors of death.

This last gloomy picture, unfortunately too real, can be drawn only when the disease runs an unimpeded course; for when addressed by the timely application of even the gentlest means of art, it usually yields with readiness. From the great affinity, however, between this modification of bilious

fever, and that which I shall next enter upon, I will reserve the particulars of the most suitable and successful treatment for another part of this paper.

Between a simple excitement of the circulation and inflammation, the line of distinction is faint and artificial, and, in many respects, capricious; for they so closely approach, that they have one precinct and are lost in each other. In the extremes, however, of these two forms of our disease, the want of similitude is so glaring and palpable, that marks of notation seem to be called for.

Since then we must assume a point of division, too factitious and arbitrary indeed, we must announce, that the chief difference consists, not so much in the state of the general circulation, as in the topical accumulations. The action of the heart and arteries is similarly excited and increased in each; but in inflammations the vascular action is greater, and the local accumulations of blood more extensive and important. It cannot be denied, that visceral enlargements do take place in some well marked cases of simple excited fever. This, however, I presume, the pathologist will discover to be entirely dependent on a dilatation of the calibers of the blood vessels, unattended by any morbid injection or organic lesion. This opinion receives ample support from a contemplation of a simple fact, that they disappear with the excitement. Since then, we are compelled to acknowledge, that morbid accumulations of blood do take place in disorders of pure excitement, as well as in inflammations, we must base our distinctions on the extent to which they are carried. We shall, therefore, in laying down the line of demarcation, simply remark, that in the excited state the blood is pretty generally, uniformly, and equably circulated, though in some cases there are partial and trifling topical accumulations. On the contrary, there is in the inflammatory variety a considerable superabundance of blood in particular parts, with an injection of the smaller vessels, and a consecutive secretion and effusion. Having settled as well as we can the limits of these two modifications of bilious fever, we shall now proceed to examine the inflammatory variety.

This, previous to the current year, was much the most frequent and fatal variety of our epidemic. We would be led to expect this by reflecting, that scarcely any man has all his organs in a state of the same healthful energy and vigour. Many causes, continually applied, co-operate to effect a state of predisposition to local disease, latent in the common range of action, but manifest on the first elevation of excitement. I might here digress into an incidental inquiry, and examine the disputed point, "whether inflammation should be considered as the effect of previous excitement," or "whether it produces it." As this investigation would, however, lead to no conclusion of practical importance, I shall pretermit it, expressing my opinion, "*en passant*," that it should generally be viewed in the light of a product of pre-existing excitement.

I might follow the example, which has been set by some eminent physicians, and subdivide the "inflammatory variety of fever," into its acute and sub-acute forms. As, however, this distinction is intended to designate a mere difference of degree of the same morbid action, I question its propriety, and shall discard it as useless. Science has too long been encumbered by such subtle distinctions without any radical or essential difference, and should be divested of such learned jargon. This subdivision being adopted by some of the greatest luminaries of medical science, I must record my dissent with becoming respect. The acute clinical observer must have noticed that the force and malignity of disease are not determined by the time of the supervention of local morbid action; but that many cases of this variety of bilious fever, in which the visceral implications appear after the third day of excitement, are more violent in their nature than others which are presented from their commencement with all the marks of local disease. The *time* of the approach of a certain train of symptoms, indicating the existence of inflammation, being assumed as the ground work of distinction, evinces a radical defectiveness, and exposes the inutility of this arrangement.

The symptoms of the first stage of this form of bilious fe-

ver, present in their great outlines, such a close analogy and obvious similitude of lineament with those of the preceding variety, that an extended delineation would be a mere rehearsal of what has been already said. It is, however, important to observe, that signs of more severe local suffering can usually be discovered on close inquiry. Laying aside this greater violence, and the previous state of health, much acuteness and nice discrimination will be necessary to enable the practitioner to foretel the form which will ultimately be developed.

The stage of oppression lasts for a day or two, during which time continual fluctuations between chilly and feverish sensations are evident. At one time slight rigors, or cold feelings are described as pervading the body, while at another some short irregular and transitory demonstrations of reaction can be perceived. A state of regular chill soon follows this vacillating condition. This chill differs very much in duration, being sometimes so short and transitory as scarcely to be observed; at other times it is so intense as to cover the body with a death-like coldness, which lasts for several hours, or even days. Cases of this severity were uncommon; for generally after a continuance of an hour or two, complete and well marked reaction ensues.

The stage of excitement being now fully established, would run on generally for several hours, when an abatement of its violence might be discerned. Frequently, however, little or no remission would take place, until near the same hour of the succeeding day, in which the chill first made its appearance. The disease, when thus formed, would commonly pursue a uniform course for several days. It, however, often occurred in the tertian type, in which the febrile elevation would remain for twenty-four or thirty-six hours, after which a remission more or less perfect would manifest itself, and usher in on the third day another chill. This form would sometimes be distinct for a few days, when a chill would take place on the intermediate day. It usually happened in such cases that there was a difference in the intensity of the chills and consecutive fevers on these respective days; the

alternate paroxysms being alike. There were other types of less frequent occurrence, which it was important for the practical physician to notice; but as they are similar to those described in many of our works, I shall pass on without tracing them to a more interesting part of our subject.

Sometimes as soon as the general excitement was raised, inflammation would be found attending it. It was much more usual, however, not to discover the common signs of topical disease, until the second stage had lasted for several hours. Evidences of its presence were often discoverable before the abatement of the first febrile paroxysm; though in many instances it did not exhibit conclusive proofs of its existence, until the second, third, fourth, or even some later rise of fever. When the stage of reaction is once confirmed by an increased and rapid circulation, organic weakness and decay, if existing, are speedily announced. The shades and degrees of predisposition are so various and illimitable, and the power of resisting local accumulation so different in different persons, and the different organs of the same person, that we should be forced to expect, at a first glance, that inflammation would occasionally occur at any period of our disease.

If after an attack of bilious fever, the febrile phenomena appear in a violent and aggravated form, we may safely infer the existence of inflammation in some organ, and to designate this organ we must look for some coincident functional derangement. The symptoms, which are common to every case of inflammatory fever, let the local suffering be seated wherever it may, are vascular activity, dry, foul tongue, great heat, difficult and anxious respiration, and great general oppression. The pulse imparted very different sensations to the finger, in some it was quick, hard, and vibratory; in others it was full, strong, frequent, and uniform; in a third class it was preternaturally large, slow, and tensive, while in a fourth it was jarring and fretful. In some it intermitted regularly, after a certain number of pulsations, while in others its intermissions were irregular and capricious. In fine, all pulses, observed to take place in inflammatory fever, occasionally presented themselves. The tongue was usually

on the first day of the second stage, covered over with a white coat, this was, however, often invested with a thick, viscid saliva. Its appearance was generally changed as the fever progressed, being converted into a yellowish or dark colour. The white aspect was observed in some cases to remain on the edges, while a dark streak was traced along the middle. Some alteration took place in nearly all patients who had been but feebly and inefficiently evacuated. It was very common as the disease advanced without any control, for the tongue to become dry, parched, and coated, and in those cases where the head and lungs were interested, it was often covered with a thick and black scurf. The tongue exhibited other appearances less common, as but little deviation from its natural aspect, a leaden hue, or an unnatural redness.

An intense and universal elevation of temperature was usually present, attended by a great dryness and harshness of the skin. These symptoms were in some confined and partial. The cheeks were hot and flushed, and the eyes dull and red, the breathing short, quick, and compressed; the expression of countenance anxious and alarmed, the sighing great, and the jactitation unceasing. General prostration of strength prevailed, with transient pains in different parts, an oppression about the præcordia, and great gastric irritability, with more or less heaving and vomiting. These symptoms may be considered as common to every species of inflammation.

It must be very obvious that a train of peculiar and characterising symptoms, pointing out the seat of local suffering in all cases, must take place. Whenever several organs sympathise, which often happens, the disease exhibits a very complicated and diversified aspect. The diagnostic symptoms of the different seats of inflammation can be given only in outline, and of course, a succinct mention will be made of those alone which are the most common and important.

The parts which suffered most frequently in our bilious fever, were the brain and its investing membranes; the lungs and their appendages, thoracic and tracheal; the liver and spleen; the stomach and intestines; the spinal cord and

its coverings; the peritoneum; the kidneys and the remaining viscera of the abdomen.

The brain being the part which was most frequently attacked, and the most important from its relations, functions, and texture, claims the first notice. In addition to the symptoms just described, we may enumerate the following, when the cranial contents are conspicuously interested. A deep and severe pulsating pain in the head; a throbbing of the temporal and carotid arteries; pain in the bottom of the orbits; an injected and fiery look of the eyes, with great sensibility and intolerance of light; and tinnitus aurium are among the first symptoms. Obstinate watchfulness, intellectual confusion, delirium, and coma now quickly ensue; and as the disease advances without obstruction, the brain is oppressed, which causes a suspension of the exercise of all its functions. The lungs no longer decarbonize the blood, the tongue, of course, becomes dry and black. The brain receives no impressions with its wonted facility, but an indifference to surrounding objects takes place. Great hebetude in all the senses is now present; the eyes are insensible, or nearly so, with dilated pupils; the ears refuse their customary office; and the sense of touch is nearly lost. An altered and faltering voice; tremors of the hands; dull moaning; petechial eruptions; discharges of black blood from the nose or other parts; small, dark stools, involuntarily passed; stertorous breathing, and convulsions soon close the tragic scene.

The approach of pulmonic inflammation was, in some instances, marked and insidious, though it usually came on with openness and violence. A pain in the chest, increased by its expansion, an oppressive load, and sensation of constriction, laborious and checked breathing, a dread of suffocation painted in the countenance, an expansion of the nostrils, a heaving of the thorax, a dusky red or black tongue, and a dark and livid glow of the cheeks, with staring and prominent eyes indicated its presence. When the tracheal and bronchial tubes were involved, there was little to distinguish it from an ordinary catarrh, except the constitutional disorder and coincident organic derangements. This form prevailed mostly in winter.

In many cases, although abdominal inflammation plainly existed, the symptoms were not defined with sufficient distinctness, to enable the physician to decide with certainty as to its seat. We cannot readily effect a decision on this point, for the inflammation of the stomach, bowels, and other parts of the abdomen, has so many symptoms alike, that a diagnosis is difficult and fallacious. This difficulty and perplexity are, moreover, increased by the inflammation extending from the organ first affected to contiguous parts, for it is seldom confined exclusively to any one. It is, however, fortunate indeed, for us, that we can conscientiously relieve ourselves from all useless and embarrassing distinctions; for it is generally sufficient for all useful practical purposes to know that inflammation actually exists in some part; for be it seated wherever it may, it is to be addressed by the same general principles, and removed by similar remedies. Without being very punctilious in searching for the genuine seat, we should permit our attention to be arrested and directed to that quarter where disease seems to be forming, without withholding it from adjacent and other parts.

If inflammation makes an invasion on the stomach or intestines, or the peritoneal lining of the abdominal cavity, it is demonstrated by a soreness, or pain on pressure so severe, as to cause the hand which makes it to be arrested by the patient, and to throw the features into an expression of great anxiety and distress. This pain is increased whenever the muscles on the anterior part of the abdomen are brought into motion. Unusual nausea and vomiting, great heat, and an uncontrollable desire for cold drinks are present. The patient lies in a supine posture, with his knees elevated, and his feet drawn up; and is troubled with eructations and flatulency. When the inflammation is confined to the stomach and bowels, we may see sometimes a little arterial blood in the mucous matter vomited, or the dark slimy stools.

Were we to peruse with credulity, many of our refined and highly wrought histories of bilious fever, we would be

led to believe, that pre-eminent hepatic disease was essential to its very nature. The inexperienced student will repeat to you what he has read, or what he has heard, and deem it heresy to express a doubt about doctrines venerable for their age: while many practising physicians, unable to dissolve early associations, still view an implication of the liver as an inseparable attendant on bilious fever. It is time, however, to dissolve this association, and enlarge our pathological views, which cannot be correct without being comprehensive. Dr. RUSH remarks in his *History of the Yellow Fever of 1793*, that he was “surprised to find so few marks of hepatic affection.” In other authors too, we can see beams of light breaking through this cloud of prejudices and early associations. Dr. ARMSTRONG in his able and luminous volume on typhus fever, informs us that he dissected two subjects who had exhibited from the commencement the strongest symptoms of hepatitis; but “on dissection no marks of inflammation were found in the liver.” His solution of this phenomenon, is in my opinion feeble and unsatisfactory. It would be much more consonant with truth and sound pathology, to say in these cases there really was no inflammation. The unnatural and morbid secretion of bile in many cases, has led to this doctrine, copied without examination by one from another, until it has become consecrated by its age. Could we with any definite and enlightened views of the laws of secretion, expect to find a superabundant discharge of bile if the liver were inflamed? Does this state of things occur in idiopathic hepatitis? Would it not be an anomaly in the laws of secretions? I am conscious that the cases of the liver and other organs are not strictly analogous; the veins in the one, and the arteries in the others being the secerning vessels. I am, however, willing for the present, to base my opinion on the different states of the liver, which are observed to obtain in bilious fever, and hepatitis. I shall in this place, only throw out some hints, which I hope may lead to a discussion, and elicit the truth. The very structure of this viscus should long since have led to the unfolding and exposure of some of these errors. Every anatomist knows

that the liver has but a sparing and parsimonious provision of arteries, and that these arteries are designed for the nourishment of the organ, and not for the secretion of bile.

Now what would be the effect of an injection and enlargement of these arteries by inflammation? They would surely, in consequence of the minute intertexture, and collateral situation of their enlarged capillary ramifications with the veins, make encroachments on their calibers, and impede both secretion and circulation. This happens in those cases where ascites is combined with hepatitis, a very common complication. The circulation of the blood, carried from the intestines through the liver, its only route, is so much obstructed by this pressure, in cases of hepatic affection, after intermittents and bilious fevers, as to be felt by the extremities of the mesenteric arteries, which become dilated and inflamed, and throw out by secretion the water which collects. Now I believe that the tenderness in the hypochondriac region, and the abundant secretion of the bile in many cases of bilious fever, are to be ascribed to a slight enlargement or turgescence of the veins. The liver is most abundantly and amply supplied with veins; I believe it will be conceded that the secreted fluid is proportioned to the number and size of the vessels which separate it. The secretion goes on in a certain range of action, which I shall not attempt to circumscribe; but when this range of action transcends certain limits on either extreme, the secretory process is suspended, and congestion is apt to ensue. Dr. AYRE, in his work on marasmus, has fully and satisfactorily shown that this accumulation in the liver will ultimately be followed by an increased secretion; and we would be led to infer as much from what we observe to take, in the arterial system, when its tubes are enlarged, and aqueous secretions are thrown into the head, the thorax, or belly.

Whenever the liver is attacked with inflammation, the patient is distressed with a fulness or load in the right hypochondriac region, attended by a pain in the shoulder, a yellow and turbid colour of the skin, and a clay-like appearance of the stools.

Other abdominal viscera, as the uterus, the kidneys, and the bladder were sometimes inflamed. These were characterised by a pain in the regions, which they respectively occupy. The secretion by urine was suspended in few instances without any pain in the loins; a pain in this quarter, however, more usually attended any affection of the kidneys.

Inflammation was sometimes seated in the spinal marrow and its investing membranes; pain in the neck and back usually indicated its existence. By pressing on the spinous processes of the vertebral column, we can find its precise seat by the suffering of the patient. In some cases a great soreness on every part of the general surface, or a peculiar tingling or crampish pain in the extremities, and a stitching feeling about the pit of the stomach were distressing.

For the sake of clearness and perspicuity, the cephalic, thoracic, abdominal, and other inflammations have been separately delineated; but we are not to infer for this reason, that they were commonly presented in this distinct form. Whenever inflammation attacks one part, it is apt to be spread continuously to the adjacent parts; or to excite such vascular commotions, as to involve finally distant organs. This gave to our disease an appearance so diversified and complex, that it was with difficulty the physician could be brought to believe that he was attending one and the same disease.

In those two forms of bilious fever, which I have already portrayed, it will be remembered that the action of the heart and arteries was increased; but in the congestive form, which I am about to define, a striking diminution often took place. This difference in degree of arterial action, in conjunction with an elevated temperature in the two former, and a low temperature in the latter, constituted the most obvious external marks of distinction between the excited and congestive varieties. Pathological investigation and the diligent researches of the dissector, have, however, shed a light on internal changes, which will more fully direct our decision. The common forms of excitement and the nature and seat of disease in them, have been dilated on sufficiently; and while, as has been demonstrated, the arteries suffer most seriously

in them, in congestion the veins are very evidently the seat of sanguineous accumulations. In the commencement of most of our cases of excited and inflammatory fever, a degree of venous accumulation, more or less manifest, prevails; but as this always stops before it arrives at the point of engorgement, whenever the stage of reaction ensues the circulation is more generally equalized. When, however, the cause of bilious fever is applied to a person on whose system are engrafted the predispositions, inviting the accession of extensive venous congestion, it really takes place to such a degree that all efforts at reaction are feeble, partial, and transitory, and vanish and lose themselves in a state of confirmed engorgement. We must observe then, that the modification of bilious fever, of which we are now speaking, differs from the two first in the occurrence of a more engorged state of the viscera in the stage of oppression, which state is continued, and prevents the evolution of an open form of fever. We therefore have seldom a distinct chill, and the third stage, or that of exhaustion and collapse, comes on without the intervention of the stage of excitement.

The most common organs affected by these almost stagnant accumulations of blood, are the brain, the liver, the spleen, and the lungs. It is very true that other parts are not exempt from their presence, but as I intend, in pursuing this subject, to speak of congestion generally, without specifying its location, and to exemplify it without tracing its faint shades of difference, “in extenso,” when existing in different organs, and may be excused from particularising any more.

The approach of the most violent and dangerous attacks of this form of fever, is often very sudden, but marked by signs which will commonly enable us to distinguish it with facility from the other varieties. The attacks are attended by an extreme and universal lassitude and weakness, and an inability in the extremities to support the body with their wonted firmness. The head is confused and vertiginous, affected with a deep and dull pain, and overpowered by the sensation of an oppressive load. The eye is heavy, watery,

and vacant, either with or without much redness; the vision is glimmering and indistinct, and when directed eagerly to an object, it gives the countenance a wild and fatuous stare. The face is dingy, or has a muddy paleness; and the skin is withered and relaxed, with or without moisture. The pulse is frequent, low, and indolent, or struggling, unresisting, and variable. The temperature is low over the whole surface, or the heat is partial and confined to particular places. The articulation is slow and drawling, or imperfect and stammering, with a voice more or less altered. The appearance of the tongue is at first but little changed, but it at length becomes dark, brown, or black, especially in those cases where there is smart vascular action in the first stage. The stomach is often irritable; the epigastric region is inflated; the bowels are usually torpid, containing dark and offensive fecal matter. The sensibility of the surface is impaired, so that blisters, when applied, act not at all, or very defectively. The mind is generally confused, and sinks at last into a state of deep stupor. Anxious respiration and constant sighing take place. In cases which end unfortunately, petechiæ, involuntary stools, slight and constant hemorrhages, or rather oozings of dark venous blood from the mouth, nostrils, and other parts, and gangrenous blotches on the extremities, close the scene.

It must be known to all, that those cases of the disease, which have been described above, are of the most violent grade; but as those which are of a milder and more tractable disposition, differ only in the degree of force, and are to be relieved by the application of the same principles of treatment, I shall pass them without a separate consideration.

The duration of this form of bilious fever is very various, as we would be very naturally led to suppose from a contemplation of its pathology. We would, on a "prima facie" view, be induced to conclude that its length would be settled by the degree and extent of the existing congestions; and we will hardly find, I presume, on a less cursory and closer scrutiny, many exceptions in contravention to this as a general rule. In many cases, where the attack is introduced by symptoms of great violence, these symptoms are often merg-

ed in speedy dissolution, unless they are checked by the timely and energetic application of the expedients of art, and ultimately relieved by a removal of congestions by ordinary means. In some milder and less inveterate cases, however, we will find the system bearing up against the depressing influence of these morbid accumulations for several days, and finally triumphing over them.

In the most violent and malignant attacks, death comes on with all the suddenness of an apoplexy. Some are at a loss for a satisfactory solution of this, and consider the effect as not corresponding with the cause. Let us pursue this subject a little further, and see what would be likely to follow an engorgement, or choking up of the blood-vessels of the brain. From the experiments of Dr. MONRO and others, we are taught to believe that the substance of the brain itself is incompressible. If then these experiments are not deceptive, an accumulation of blood in one part of this organ, must take place at the expense of some other part, for the cranial cavity cannot contain more at one time than at another. Supposing then that the quantity of blood in the veins is increased, a corresponding diminution must take place in the amount of space occupied by the arteries; the diminution of the one being proportioned to the increase of the other. When the accumulations of blood in the veins are great, causing an obvious and excessive distension, their natural and unvarying consequences, will be such encroachments on the arteries, as essentially to impede, if not to suspend the transmission of blood through their tubes. We will, however, argumentatively allow, that the conclusion from the experiments to which I have alluded is incorrect, and proceed to examine this point, taking it as conceded, that the cerebral mass is capable of being compressed. Now in this case, the brain itself would be overpowered by the pressure of the enlarged veins, and the sensorial functions destroyed. The arteries too would co-operate to produce this result, for the free passage of the blood being prevented by the very nature of congestion, the force of the arterial circulation would be brought to bear on the point of obstruction. For practical

purposes, I conceive it to be unimportant, which view of the subject we adopt, for the consequences and the treatment would be the same; and I shall, therefore, in compliance with general usage, speak of an oppressed brain.

When the brain is oppressed by congestion, its mobility is soon destroyed and its functions cease: and as all the other organs are dependent upon this for nervous power, and a due and healthful exercise of their offices, they become involved in the same ruin. The energies of the system are paralyzed; the animal heat departs; secretions no longer go on; the blood does not undergo by respiration the necessary changes; the mind cannot continue its operations, but is gradually lost in a state of indifference and profound stupor. The diaphragm and heart cease to act; and all indications of vitality soon vanish.

In more violent cases of inflammatory fever, with symptoms of cerebral implication, the similitude of symptoms, with those which occur in congestion, is often striking, and obscures the marks of distinction. We must attend closely to the history of the case, previous to the supervention of coma, to aid us in arriving at a safe and correct conclusion. An open and high fever, followed by a delirium, and as the disease advances, and its symptoms increase in violence, by an insensible and comatose state, will furnish a good diagnostic mark, the existence of excitement after the first stage, giving ample grounds for distinction. But were this mark of difference taken away, we would be very much embarrassed in some cases in deciding, for the symptoms of pressure on the brain are alike, whether proceeding from inflammatory or congestive action.

It was from mistaking the oppression, for exhaustion or debility, which prevailed in the system in some cases of the inflammatory, and generally in the congestive forms of our fever, by a few physicians, that a controversy arose among the members of our profession in this part of our state, which I shall notice succinctly when I come to speak of the most successful treatment in congestion.

The history of the different forms of the fever, as it is re-

corded in the preceding pages, with the pathological remarks, which I have made, will I hope enable the experienced and enlightened physician, to settle definitively in his mind, the character and nature of that disease which has prevailed so extensively among us. Whenever the causes and nature of a disease are perspicuously and faithfully delineated, little need be said to aid the scientific physician in deciding on the course of practice best suited to its indications of cure. Our general principles of treatment are so plain, simple, and comprehensive, that it is almost superfluous to append to the history of a disease, a detailed account of all the remedies employed for its removal. I shall, therefore, only offer a brief outline of the means which proved most efficient in my hands in relieving the sick.

The two first forms which were noticed, show a resemblance so striking, and an affinity so complete, that I shall consider their treatment at the same time. Between the worst cases of the simple, and the least complicated and mildest examples of the inflammatory forms, the division is so trifling and unimportant, that nothing more is required for practical purposes, than a slight difference in degree of the activity of the remedies employed.

The first remedy which I shall notice is venesection. In a large majority of the cases of simple excitement, blood-letting could be very safely dispensed with; however, when any hardness or strength of the pulse seemed to indicate its use, I did not hesitate to resort to the lancet with caution. I believe that its cautious employment judiciously suited to the existing state of the circulation, often prevented the approach of inflammation, facilitated the operation of other remedies, and shortened the progress of this form of the disease.

It was, however, in the inflammatory form that its advantages were most decided and unequivocal, and its employment often indispensable; for it secured organs in many instances from lesions, which would have resulted from the great arterial action, more speedily and effectually than could otherwise have been done. I bled in every exacerbation of the disease after the chill, if the arterial activity required

prompt reduction, which was indicated by a tense or corded pulse. The good effects of the lancet, where used to the extent of making an impression on the pulse, which I generally attempted to accomplish, were often sudden and astonishing. It not only co-operated with the other remedies used, in reducing the excitement, but generally rendered their operation easy, prompt, and efficient. When blood is abstracted in such quantities as to make the system acknowledge its loss, cathartic medicines will generally operate kindly and copiously. I have several times been compelled to bind up the arm, and hasten from the room, my patients calling for the close-stool; even such as could not be operated on effectually while the arterial action remained unchecked. I was sometimes forced to take only very small quantities of blood at a time; but the abstraction of these sometimes brought the most obvious relief where a tension of the pulse remained in the advanced states of the disease. I cannot trace all the good effects, caused by the use of the lancet; but shall merely add, that it was a most powerful means of removing the disease itself, and, of consequence all the distressing symptoms, many of which disappeared soon after its employment.

*Purgatives* were, however, the most important and indispensable medicinal applications in every case. An almost exclusive reliance was placed in their curative powers, in all cases where the lancet was omitted, and they proved equally as beneficial as venesection, where both remedies were synchronously used. They removed the acrid and stimulating loads of fecal and excrementitious collections, which would have been constant sources of serious irritation. They, however, probably accomplished much more by their salutary operation, in reducing by a steady and well-timed depletion, the increased action of the heart and arteries, and bringing it within the circle of health; and in thus restoring it to its accustomed range, the natural secretory actions were renewed, and all the functions of the different organs re-established. In fine, by their co-operation in removing the disease, all morbid symptoms vanished, and the healthful energy, and harmony re-appeared.

The cathartics which I chiefly employed were calomel, rhubarb, castor-oil, jalap, aloes, the neutral salts, and the tartrate of antimony in various combinations. In inflammatory fevers, it is necessary, previously to the administration of purgative medicines, to have recourse to the lancet, in order to insure their full operation. I believe, that the first of these cathartics should never be given in cases of high vascular action. I generally, when called in the second stage, (and I was seldom called before,) drew blood until I made a sensible impression, if the pulse was full or strong. As soon as my patient revived after the blood-letting, which, in violent cases, I sometimes repeated in an hour or two, I usually gave a cathartic, composed of calomel and rhubarb or jalap, which generally operated repeatedly and copiously. If it was necessary to resort to any auxiliary measures, the neutral salts, in divided doses, answered in most cases extremely well.

I gave my purgatives with a wish to keep up if practicable, "*currente morbo*," a continued and unceasing discharge from the bowels. For the accomplishment of this end, I usually exhibited them in such a manner, that the operation of any one would support the operation of that which preceded it, unless this was strongly contra-indicated. I, however, cautiously avoided any active purgation about the time of the expected chill; and generally gave medicine sparingly for a short time before it came on. In the rise of fever which ensued, if it was not very high, I repeated the mercurial cathartic, and assisted its operation by the sulphate of magnesia or the sulphate of soda, given in broken doses every two or three hours, directing my patient to be governed by the effects, and not by the quantity taken.

Objections were often very strenuously urged by some physicians to the use of calomel. That it was often abused, I am conscious, but this is no argument against a cautious and discriminating use of this medicine. One of their objections to it was exceedingly puerile; they said that it caused a great increase of the secretion of bile, considering the bile very vaguely as the cause, and not as the consequence of our

fever. Now supposing the liver to be in the state which has been described above, is any thing so well calculated to bring relief as a free secretion of bile? which, in fact, often takes place sooner or later. Some very enlightened members of the profession objected also to the use of the neutral salts, as bringing watery discharges alone. On a close examination, however, of this objection to their employment, we imagine that it will be considered as groundless. I believe them to be improper and inadequate in congestion, and in the advanced and collapsed stage of excited and inflammatory diseases; and I suppose it to be owing, in some measure, to their abuse in such instances that prejudices have been raised against them. These instances require more warm and searching medicines; but in the reactive stages of the excited and inflammatory forms, nothing is in my opinion more suitable and judicious. It cannot be denied that they bring away aqueous stools, but are not these stools detracted from the mass of circulating fluids? This circumstance, connected with their cooling and refrigerating nature, renders them extremely proper in those cases where a reduction of heat and arterial action is desired. I do not, even in these cases, advocate their employment exclusively, but only as an adjuvant to calomel and other more active cathartics.

Calomel, rhubarb, aloes, gamboge, and other purgatives in various combinations were given, as they seemed to be indicated, in such doses and at such intervals as to excite and keep up in many cases throughout the disease, a steady discharge of black matter.

Large cathartic injections, frequently administered, were very useful in promoting the action of the purgatives. They were also attended with the most obvious advantage, by removing from the large intestines all morbid secretions and irritating feces. Injections of cold or cool water, by lessening the heat, were often attended by the most beneficial results, and pleasant and comfortable feelings.

Partial ablutions, more or less extensive and continued, according to circumstances, for a longer or shorter time, were a powerful subsidiary means in reducing the heat and

other febrile phenomena. They would act in some cases almost as promptly and effectually as a small bleeding, and being very commodiously applied, and very grateful to the feelings of the sick, they should be omitted in no cases where the general heat is above the standard of health.

The above measures were generally sufficient to bring on a solution of fever; in those cases, however, where confirmed topical disease took place, it was often advisable to give calomel in such a manner as to make a slight mercurial impression about the close of the fever. Whenever it was excited, either by accident or design, before the declension of the febrile symptoms, it was of serious disservice. A proper mercurial action and the application of blisters, after the reduction of the inflammatory symptoms, generally perfected the cure.

As the depletory measures of bleeding and purging were carried into effect, the chills would become milder and often disappear. But it would be found requisite, sometimes, when the fever was almost subdued and the stage of collapse was approaching, to use wine, bark, laudanum, and other tonics, with sinapisms and blisters to prevent them. In the third stage it was necessary, in many cases, to do little or nothing, in others, however, I was forced to use a moderate tonic course, or even to support the patient by a liberal allowance of wine and corroborants. The mildest food in small quantities should be given in this period of the disease; and as the patient convalesces, the return to a full diet should be guarded and gradual.

I have said nothing of the treatment in the stage of oppression, because I was seldom called in before it had passed. Purgatives, however, assisted the disease to emerge, and were almost the only medicines used. In every stage the accession of cool and fresh air is to be recommended; but it will be found particularly useful in the second stage, when the patient should be lightly covered, and the whole of the antiphlogistic treatment rigidly enforced.

In the simple and inflammatory varieties, an obvious and regular excitement exists; but this cannot be said of the con-

gestive form, in which the evidences of reaction are entirely absent, or partial, irregular, and transient. Almost every fever, as has been said, has a stage of oppression, however short it may be; but those cases alone are truly congestive in which this oppression is great enough to suppress all efforts at regular excitement. The oppression is in some patients very strongly marked in the beginning, but suddenly receding, it is followed by an open excitement, which runs its course in one of the first described forms.

In the most violent examples of the inflammatory bilious fever, in which depletory measures were neglected for a time, the patient would sometimes sink into a state of stupor from vascular fulness, attended by symptoms of great seeming weakness, and, in many of the cases of congestion, as well the milder as the more urgent, the greatest apparent debility existed. It was, however, only apparent; though it was mistaken for real debility by some physicians, who excited a controversy in support of their opinions, which raised curiosity, and made some noise even out of our state. As, however, this controversy was carried on in the novel and indecorous channel of the newspapers, it has never been presented in a satisfactory shape for medical adjudication. As I lived and practised in the county, and lastly in the very vicinage, where some of the most violent and stubborn cases are said to have occurred, I will attempt to gratify curiosity by presenting an outline of this dispute to the tribunal of the medical public.

The stimulating, or, as they were improperly designated, the Brunonian physicians, mistaking therefore the oppression, which existed in the congestive, and sometimes in the inflammatory forms, for actual debility, extended by a very easy transition, their views to most cases of the disease, thus accomplishing for themselves an important and desired generalisation. It is true, that in all the forms they had some colour for their error; for every febrile excitement produces seeming weakness and prostration. Acting, therefore, in accordance with these views, and conceiving that they had a disease of actual and not seeming debility to treat, they pur-

sued a stimulating practice; and believing this debility or exhaustion to be most abject and excessive, they entertained the crude and mechanical opinion, that it was to be removed only by the most prodigious and colossal doses of stimulants. They publicly announced their principles and practice, and regardless of the delicacy of professional courtesy and claims, they conjured each other not to consult with the depleting physicians, denouncing them without discrimination as unsuccessful in their practice. This course, and these charges were noticed, as might have been imagined, and repelled in a proper and becoming manner.

From the exposition of the nature of the disease, which I have given in the preceding pages, the results of such a treatment may easily be anticipated. Patients labouring under coma, were by the remedies employed, acting in unison with the disease, plunged into more profound coma, and increasing the oppression, the chief source of danger, hurried on to death. Sometimes, however, such cases did not eventuate fatally, for the stage of collapse would come on in which stimulants were proper, and the continuance of this course successful. But, indeed, it much oftener happened, that those patients who were treated on this plan, even if affected with a simple and open form of fever, were thrown into an algid and comatose state. Theoretical reasoning would lead us to conclude, "a priori," that this state would result from inordinate stimulation; and experience demonstrates most abundantly that it generally occurs. We had most positive and convincing evidence, in many examples which came before us, to establish this point; but even setting these aside, we might infer with logical certainty, from the over-proportion of comatose cases which fell within the circles of the Brunonian physicians, that many of them were factitious. If this insensible and comatose condition be ascribed truly to a vascular fulness and turgidity; every thing which increases the action of the arteries, every stimulus, morbid and medical, must combine with the forces already in play, and augment the malignancy of symptom and the disease. They resorted to the most unparalleled degree of stimulation: Brown him-

self would have shuddered at their excesses, and frowned at this gross perversion, and irrational and unscientific application of his principles. They administered bark and wine, because, in their imaginations, they were indicated; these, however, often increasing the disease, the patient was conceived to be in a sinking state, and larger doses were given. These too, in their turn, attended with the same or similar consequences, were followed by still larger doses. A state of collapse, subsequently remedied by stimulation, in a few instances, ensued; but much oftener some organ important to life was thrown into a state of irretrievable disease.

Having given a fair and candid view of the theory and practice of one set of physicians in our epidemic, I shall now describe the treatment I adopted in similar cases of it. A little attention to the nature of the congestive form, will convince us that nothing is so well calculated to release the blood-vessels and bring again into healthful action, as a judicious use of venesection.

There was, as it were, a stagnation of the circulation, and by the abstraction of blood an impulse was often given it, which brought on a renewal of its motion. The blood could not always be made to flow, as soon as the veins were opened, but often after it had issued from the orifice by drops, or trickled down slowly, as the circulation was relieved and became freer, a large and full stream would be supported. It was sometimes necessary to use warm applications, before we could obtain a sufficient quantity. A large quantity could be drawn with the most evident good effects, whenever the pulse became more lively, and the other symptoms less urgent, while it was flowing. It was not safe, however, to bleed largely in every instance, for a reaction would not always follow. In these cases the loss of a few ounces would indicate the propriety or impropriety of a repetition of the operation. A slight impression might be noticed, which would direct us on this point; if the pulse became rather more open, with a mitigation of the other symptoms, the bleeding might be repeated with advantage; but if these desired changes were not observed; if, after the first operation, the pulse became

weaker and more frequent, and the restlessness and anxiety greater, the loss of more blood was hazardous. A mixed course of purging and tonics in such desperate cases, afforded the best prospects of relief. Coma was sometimes relieved very speedily by venesection alone.

But the simultaneous and auxiliary application of blisters, sinapisms, and the partial warm bath, was eminently salutary. They brought on a warmth of the extremities, and restored the circulation in them. I sometimes applied warm bricks, or stones, or bottles of warm water, very conveniently, with obvious success. The gently excitant power of friction also co-operated to restore the circulation.

Cathartics, as soon as they could be administered, were always employed. From the general torpidity of the intestines, it was necessary to give them in large doses, and to repeat them often, in order to procure their copious operation. Calomel, combined in various proportions with rhubarb, aloes, jalap, gamboge, and other active medicines of this class, repeated according to circumstances, and aided by frequent enemata, generally brought on the most plentiful and copious discharges of dark or tar-like feces. When evacuations of this kind once presented themselves, it was often very easy to keep them up, by giving compound cathartics, as recommended, as long as any evacuation was needed. These medicines, independently of their clearing the "*primæ viæ*," of offending matters, and removing febrile action, frequently placed the system under the mercurial impression. When this happened, recovery invariably ensued, if proper attention was paid subsequently to the state of the bowels. No plan of treatment can keep the blood-vessels in that smooth and even tenor of action, which approaches the healthful, with as much certainty as a slight salivation, assisted by blisters. After the removal of the insensible and comatose state, and the release of the system from oppression, the indications were plain and obvious. The first passages were duly attended to, and general principles directed all other prescriptions.

I might here close my observations on the bilious epidemic;

but as it presented during the last season some new and anomalous appearances, and some unwonted and irregular traits of character, I feel as if my duty to the public and the profession, calls on me to give it a separate and succinct notice. I shall enter on the discharge of this duty with the more reluctance, as I shall again be forced to examine and analyze the collisions of adverse opinions and practice. The triumph, which was achieved by the advocates of depletion over their Brunonian adversaries, had scarcely been recognised by popular acquiescence, and the decrees of our medical tribunals, before we saw with concern and regret, that it would be followed by a dangerous and convulsive reaction of medical opinion. The intellectual powers of man, which were wisely fitted by the great Author of our being for the discovery of truth, and devised for the direction of our actions, often lead us, without the most cautious control and rigid discipline, into gross error and the most hurtful excesses. When convinced of the inconsistency or absurdity of one extreme, it seems to be a principle of human nature grafted on our weakness and brought into daily action, to pass the "golden mean," and embrace the opposite extreme. We see daily proofs and illustrations of the truth of this remark, in the constant changes of opinions and tenets in philosophy and religion, and the revolutions in medicine and politics. This active principle of our nature requires some salutary regulations and restrictions, for without these it is sure to mislead us in our researches after truth.

"Felix qui potuit, medium cognoscere tutum."

The guise, however, which the disease put on, during the summer and autumnal seasons of the last year, I am fully persuaded, and shall endeavour to prove, was produced not so much by the disease, as by the hurtful action of improper and injudicious medicinal applications. The disease, as it occurred to my inspection and management, was of a mild and manageable temper, if adroitly and dexterously addressed; but the most scourging fatality ensued on the operation of

medicines, the force and activity of which were unsuited and over-proportioned to the violence and force of disease. Few indeed were the cases in which any unprecedented or malignant symptom presented itself, in the earlier stages, if we resorted to the gentle expedients of art alone, but death too often followed a contrary and harsh practice.

In this, as in every year since I have been conversant with disease, pure and well marked examples of all the forms of fever which have been described in the preceding pages, were to be seen in our professional intercourse. But the proportion of cases of the simple or excited form was much greater than I have ever witnessed it in previous years. The practice which I believe was generally pursued, was well suited in the outline, to the few cases of inflammatory and congestive fever which were met with; but was too active and violent for the more prevalent form of excitement. I will therefore confine my comments mostly to the history of the simple form, giving as faithful and as impartial a relation of its symptoms, natural as well as factitious, as I am able. Although some discrepancies could be discerned in the features of the simple variety, during the last and preceding years, still the great similitude and strong family likeness in the broad outline, were sufficient to prove a common parentage.

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“Facies non omnibus una

“Nec diversa tamen, qualis decet esse sororum.”

The attack was, for the most part, preceded by rather strong premonitory symptoms; for previous to the accession of an obvious chill and febrile paroxysm, the patient was distressed with sensations of general lassitude, heaviness or painful and giddy feeling in his head, nausea and irritability of stomach, loathing of food, though in some cases the appetite would be good, alternations of hot and chilly fits, weak and frequent pulse, partial action on the surface, wrinkled and contracted skin, clouded and dull eyes, with pain in the orbits, inattentive and dejected countenance, a scanty secretion of limpid urine, and a pain in the back and loins.

These symptoms having continued most usually for two or

three days, ushered in "currente morbo," a well marked cold fit or chill. During this, many of the above described appearances were increased; the features became sharpened; the extremities and other parts cold; and the gastric irritability and thirst distressing; vomiting of bile; anxious respiration and oppression about the precordia ensued, attended by a weak, small, and frequent pulse. The continuance of this stage of chill was very various, but in most instances, after it had lasted from one to three hours, it generally gave way to the febrile reaction.

As soon as the stage of reaction was pretty well established, the sickness or nausea would often vanish; the pulse would become lively and resisting; the temperature of the surface elevated; the skin dry and harsh; the lips, fauces, and throat parched, accompanied by a sensation of choking or suffocation; the thirst excessive; the cheek flushed; the head pained; the eye dull and injected; the tongue whitish; the respiration quick and anxious; and the nervous and sensorial mobility great. Mental incoherence, and sometimes delirium, alterations of the secretions, and intestinal torpidity followed in the train. When these febrile appearances have attained their acmè, and lasted for a longer or shorter period, occupying in the whole paroxysm, an aggregate of from six to twelve hours, they commonly begin to decline. The general distress in some measure vanishes, the skin becomes moist, and finally a sweat breaks forth, bringing with it comparative comfort and freedom from distressing feelings, and terminating in a remission more or less perfect.

This remission was not of long standing without proper remedial measures. Another paroxysm, longer and more severe in all its stages, ensued, ushered in by great coldness, vomiting of bile and other matters, and ultimately followed by excessive heat, thirst, and dryness of surface, delirium, difficult breathing, febrile anxiety, parched and brown tongue, and pain in the head, and about the epigastric region. The third paroxysm was still longer and more severe, the remission being less perfect, and bringing less relief, and appearing but a few hours before the fourth accession, whether the

disease made quotidian or tertian returns. In this way, if the disease was not arrested in its fatal career, each accession proved more violent than its predecessor, till exhausted nature gave way. The pulse declines, the skin becomes sallow and cadaverous, the countenance shrunk and ghastly, the abdomen swells from visceral congestion, and collections of morbid secretions and wind, the stomach loathes all food, and hiccough, petechiæ, stupor, or a low and unceasing delirium introduce death. This unfortunate picture can, however, be drawn from those cases alone, which have been neglected from the beginning, or improperly managed in their course.

I soon ascertained from an attentive view of the history of the first cases which presented themselves, and a due regard and diligent inquiry into the "*juvantia*" and "*lædientia*," that the disease, whose attacks it was our duty to counteract and repel, was one which required and could bear only moderate evacuations. Since I was convinced of the truth of this opinion, and acted in accordance with the dictates it suggested, the cases which have fallen under my notice, have been much more manageable than similar cases were in the earlier part of the season. I moreover found it necessary to resort to tonics, in order to prevent the returns of chills, and support the system, much sooner than I had previously been in the habit of giving them. In many cases of the most common form of fever which prevailed this year, we could succeed by a recourse to the gentlest expedients of evacuation by emesis and catharsis, continued for a few days "*pro re nata*," followed by a timely and judicious appeal to bark, and other tonics, to ward off the chill or brace up the powers of the system.

I found the operation of emetics in the febrile exacerbation, to produce the most salutary changes and surprising relief in the form under our consideration. The extensive control exercised over arterial action by the nauseating influence of this class of remedial agents, kept it in safe bounds and restrictions. All substances contained in the stomach, whether in the form of food, phlegm, or bile, the two last of which abounded in many cases, were thrown up without much un-

casiness, which tranquilized this organ to the great comfort of the patient. After the operation of ipecacuanha and the tartrate of antimony combined, the skin recovered its colour and softness, sweating soon ensued, and the patient was quieted by the removal of the heat, thirst, pain in different organs, and the anxious and oppressed respiration. The remission brought on by this means, appeared, in most cases, sooner than it would have occurred spontaneously, and was more perfect. Emetic medicines were also followed usually by a few full and copious operations downwards, which paved the way for the administration of cathartics.

The complete evacuation of the first passages by the *judicious* use of *cathartic medicines*, was an object in the treatment of our disease of which we could not lose sight. The removal of fecal accumulations, and morbid and irritating gastric and intestinal secretions, was always a point of primary importance; but the salutary effects produced by the operation of cathartics were not limited to the accomplishment of this end. During the existence of sthenic symptoms, the increased secretions along the tract of the alimentary canal, depleted in an efficient manner from the vascular system, changed the torpid state of the intestines into a brisk peristaltic action, released the circulation from its derangements and accumulations, and facilitated the free and easy passage of the fluids along their tubes. After the bowels had been moved pretty briskly and copiously, in the first and second febrile exacerbations, I witnessed, in a large proportion of cases falling under my notice, an extreme and unusual susceptibility to cathartic impressions. This, in fact, was a circumstance of such common occurrence, that it was usual with me to prescribe medicines in small doses. I have thought that I derived as much benefit from the action of five or eight grains of the sub-muriate of mercury, and as many of rhubarb or jalap, as I formerly derived from nearly double these quantities. The gentle and steady operation of this class of remedies was salutary during the continuance of excitement, which was observed to last variously from three to eight or

ten days; but when the discharges induced by them were pushed to excess, they were of incalculable and fatal disservice, as I shall presently show.

As in most cases of the excited variety of our disease the local symptoms were unworthy of notice, the cautious and discriminating use of emetics and cathartics, aided by cooling and refrigerating drinks, and cold affusion in the hot stage, and the adoption of the antiphlogistic regimen for several days, was sufficient to reduce the febrile actions. It was seldom necessary to call to our aid the alterative powers of mercury, the lancet, or blisters, those powerful engines in subduing the attacks of the other forms, which presented little peculiar in their nature this season.

As soon as the disease began to acknowledge the supremacy of medicine, and the arterial action had waned away, we were forced to employ tonics and stimulants with their subsidiaries, sinapisms, &c. to ward off the chill or prevent the sinking of the system, which seemed inclined to ensue. The usual medicines in such cases were given with decided advantage; and I also found that a freer and earlier resort to their employment was not only sanctioned by their success, but was often required. The proper time for exhibiting the tonic class of medicines requires the clearest judgment of the practitioner, and it is at these critical and decisive moments that real ability unfolds its resources and acuteness of discrimination, and rescues the sufferer from the jaws of death, while the blundering devotee of a routine practice signs his death-warrant.

It will be entirely unnecessary and superfluous for me to consume much time in demonstrating those propositions, which the enlightened pathologist and experienced practical physician must know and acknowledge. In the inflammatory and congestive forms, a prompt and energetic employment of free and liberal evacuation was successful; but in the excited variety, which presented a majority of cases, and scourged in a most memorable manner this quarter of Virginia, we had no local disease to require bold and decisive depletion; and it was owing to a baneful wish

to generalize and apply the same treatment to all cases, that the advantages of observation have been limited, the progress of medical improvement retarded, and many of our most valuable citizens lost.

When a beneficial result is obtained from a medicine in a small dose, we are naturally led to expect an increased advantage from an augmented dose. But as the disease before us was one of weak morbid action, the use of drastic medicines soon reduced the action of the blood-vessels below the range of health; and unless a timely revolution of treatment was adopted, brought on the most disastrous consequences. The action of the heart and arteries being brought below par, the febrile heat was soon succeeded by a coldness of the general surface, great restlessness, irritability of the stomach, anxiety and alarm. The blood retreating from the surface to the centre, the circulation through the capillary vessels was very imperfect, the skin losing as a consequence its moisture and plumpness, the eyes becoming sunk, the features contracted and sharp, the thirst excessive, and the feeling of internal heat distressing. The brain losing its wonted supply and support of blood, ceased to perform its functions with regularity and effect; the sensorial powers were deranged, the nervous influence was impaired, the mind was wandering and uncollected, and a low delirium took place. This state of disease being often mistaken for one of suffocated excitement or congestion, the activity of remedial measures and the violence of the above symptoms increased "pari passu." Large and repeated purgatives were now given for the purpose of releasing the system from an oppression which existed only in the imagination, and these sinking the system still lower, the secretory action on the villous coat of the alimentary canal was very trifling, or suspended entirely, so that at last the dejections were small, frequent, of a bottle-green colour, and totally unproductive of any relief. The pulse now became small, weak, confined, and thready, and in the frequency of its pulsations was often more than one hundred and forty in the minute, and sometimes incalculable, the wrists were cool, while the palms of the hands and

soles of the feet were hot, the skin would be insensible to the action of blisters and local excitants, and all discharges from blisters previously drawn would cease. These symptoms increasing, the muscles would become relaxed, the pupil of the eye dilated, and the stools passed unconsciously; the attention of the patient either could not be excited at all, or when roused he would answer that he "was well." The pulse would give way, the coldness get greater, and every symptom of debility from abstraction present; the patient would start, the eyes would be injected and yellow, and quiver in their sockets, and life, at last, appear to evaporate. We lament to say we have often seen these melancholy, unscientific, and fatal perversions and applications of luminous, correct, and scientific principles. We lament that we have seen a disease of high vascular action, treated on the Brunonian plan, while on the other hand, we have witnessed a disease of weak, morbid action, addressed by the boldest and most fatal excess of depletory measures.

Such was the epidemic, which has visited us annually during the autumnal season for several years past. It is reasonable to believe, that in a country like ours, so diversified in its surface, so fluctuating in its climate and seasons, so various in the artificial changes, presenting such an heterogeneous assemblage of inhabitants of all colours, and habits, and manners, this disease was very often modified by local and partial causes. We cannot, arguing from principles and experience, suppose that the bilious fever, on the flats below tide water, would be alike in every respect to that which prevails in the mountainous districts. There will be some difference, but it is such a difference as arises from the more or less concentrated, and combined, and violent action of the similar causes proved by the exhibition of the different forms of the same disease. It has long been considered as a leading principle in the science of medicine, that during the prevalence of any disease, all other maladies assume the livery of the reigning epidemic. There may, indeed, be some difference in the intensity of effect on the human constitution, produced by the like causes of disease, there may be some

difference in the malignancy of the symptoms, but the disease which has gained the ascendancy holds undisputed dominion, and all others acknowledge the supremacy of their sovereign. I am sorry to think that this principle should ever be neglected or misapplied by the practising physician. Yet the neglect or misapplication has given birth to many erroneous opinions and theories, and what is worse, to misconduct and erroneous plans of treatment.

I have endeavoured to give a full, clear, and impartial view of our autumnal epidemic. I have attempted to touch all the important points without descending to minutiae; and I hope I have not passed over any essential matters. It is only by considering a subject in this manner, that a physician can hope to master his science; partial views will never answer. The physician must learn to embrace his subject, to connect the different parts, and view these in one harmonious whole. It is only by examining on this large and comprehensive scale, that ours can justly be called the healing art, and the practitioner be considered as belonging to the noblest profession that ever employed the human mind, or blessed the human race.



Fig. 1.



Fig. 2.

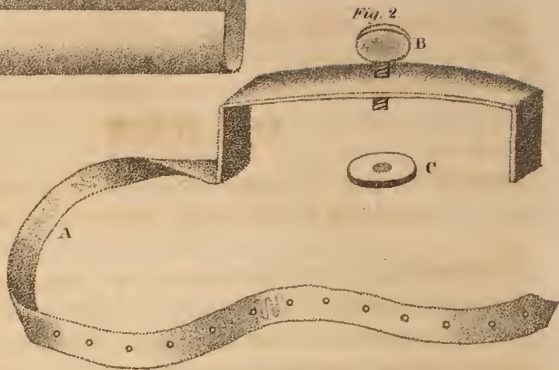


Fig. 3.

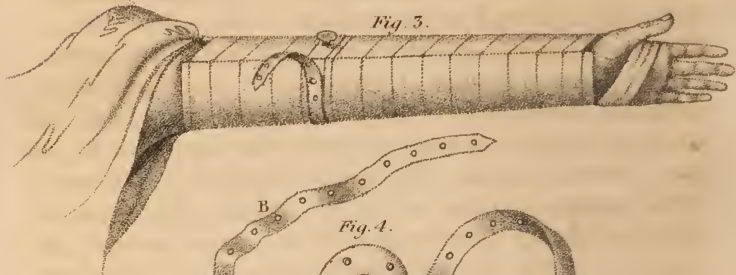


Fig. 4.

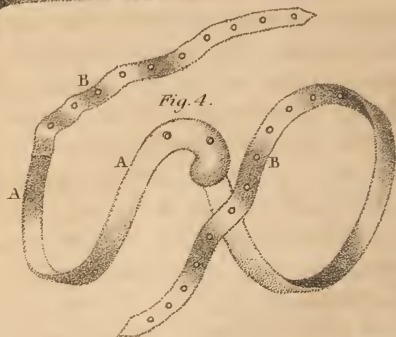
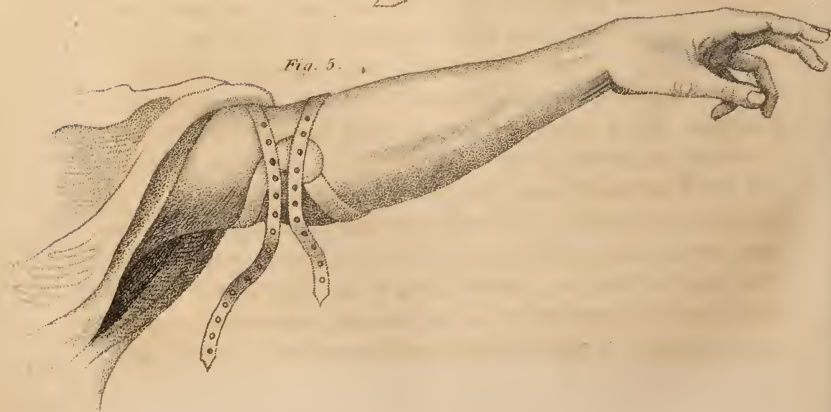


Fig. 5.



## CASES.

ART. X. *Case of Aneurism of the Brachial Artery, cured by compression.* By W. B. FAHNESTOCK, M. D.

MRS. PRICE, sixty-six years of age, was bled for a pain in her side, on the 8th of May, 1823. As soon as the lancet was withdrawn, the blood gushed forth with unusual violence, in an irregular and interrupted current of a bright scarlet colour. The operator being alarmed at the unusual flow of blood, immediately attempted to stop it, and at last succeeded after several fruitless attempts, and considerable hemorrhage, by placing several compresses upon the wound, and binding the arm very tightly. The bandage occasioned great pain during the night. The next morning it was removed and re-applied, and kept on for two weeks. The same day after the bandage was removed, the arm began to swell, and became of a black and dark purple colour about the wound, and this appearance gradually extended on the internal part of the arm, as far as the wrist. A few days after this she discovered a small tumour, about the size of a pea, which gradually increased and began to pulsate strongly; she now consulted Dr. GRIFFITH, who told her that the artery had been wounded, and advised her to consult some surgeon, but which she neglected to do. It gradually increased to the size of a large filbert, and pulsated very strongly. She now became alarmed, and consulted, (then my preceptor,) Dr. PARRISH. Dr. Parrish applied the usual bandage, which consisted of a tin from six to eight inches or more in breadth, and about two feet in length, bent in such a manner as to be adapted to the back of the arm, (see the plate, fig. 1.) This was lined, and applied to the back of the arm; a roller was then applied over it, commencing at the fingers, and gradually extending up the arm to the tumour, over which a piece of lead about the size of a fifty cent piece, and several com-

presses were applied. The roller was then continued over the compresses, and bound somewhat tightly, after which it was continued up to the end of the tin, where it was fastened; the arm was then kept at rest, she was purged occasionally, and lived upon a low diet. This bandage was to be removed and re-applied every morning. This was done by one of my fellow students, (now Dr. J. H. YARDLEY,) for about three weeks, after which time he took sick, and the case was transferred to me. I continued to dress it for about six weeks, and the tumour was not much smaller then than when I first saw it. About this time Dr. Physick saw the case, and recommended her to keep the arm elevated above her head, at the same time that the bandage was applied.

Every morning upon removing the bandage, I observed that the lead had always shifted, and was sometimes quite off the tumour, although it had been accurately applied the preceding morning. During my attendance, I devised a machine which compressed the tumour, at the same time that it prevented the lead from moving. A correct drawing, (fig. 2. A. the strap, B. the screw, C. the lead,) will answer better than any description I am able to give of it. (Fig. 3. machine, tin, and bandage applied.)

The machine was applied and kept on for ten days without removing it, at the end of which time the tumour was very considerably lessened, and I have no doubt had the use of it been continued, would have effected a cure in a very short time, but having worn the tin so long before the machine was applied, she became tired and discouraged, her circumstances not admitting her to lay by any longer, its use was discontinued and a simple bandage applied in its place.

I saw the arm several times during the winter, and in the spring of 1824, the tumour had increased to the size of a hickory nut; she still refused to have the machine applied.

In April, 1824, I contrived another instrument, which allowed her to use the arm, at the same time that it compressed the tumour, (fig. 4. AA. steel, BB. the straps.) This machine was applied, and kept on without removing it, until the cure was completed, which was in March, 1825; she wore

the instrument without the slightest inconvenience, and worked as she did before she received the injury. It has not been re-applied since last March, and as yet there has been no return of the disease, although she works very hard. (Fig. 5, the instrument applied.)

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ART. XI. *Case of Inversion of the Uterus, with Disruption and Adhesion of the Placenta.* By WILLIAM CHURCH, M. D. of Pittsburgh. Read before the Pittsburgh Medical Society.

AT six, P. M. on the 14th of July, 1824, I was called by my friend, Dr. HANNEN, to visit a poor woman in Pitt township, Alleghany county: the history of whose case was as follows. About four, A. M. on that morning, after having been in severe labour the whole of the preceding night, she was delivered of her second child, half an hour before the arrival of her midwife. The midwife, on her arrival, endeavoured to deliver the placenta; but, having persevered in her fruitless attempts to deliver it, until eight, A. M. she sent for a physician. The doctor told them that "the retention of the placenta was of no consequence, that it would come away of itself, after the woman became stronger," and took his leave without *ascertaining its state by an examination!!* The midwife waited until two, P. M. for the placenta to be thrown off, and then attempted to effect the delivery by main force, by pulling at the funis umbilicalis until she brought away about half of the placenta and inverted the uterus, when profuse hemorrhage immediately supervened. The remainder of the placenta being adherent to the *fundus uteri*, she forcibly drew it and the *fundus uteri* through the *os externum*, and fixed a ligature on the adherent placenta, which she fastened to the woman's thigh; she did this, she said, "to keep it from slipping up out of reach." Being now much alarmed, she sent for the physician who had visited them, who "was sick, and could not come." Dr. Hannen was sent for,

and on his arrival, finding the woman's situation truly deplorable, gave her an anodyne, and a tea-spoonful of ether every half hour, and requested that I should be called in consultation.

On my arrival she was without pulse and unable to speak, her extremities were cold, respiration irregular and difficult, and her countenance completely bloodless: she was very restless. The bed and floor were covered with blood. The uterus was completely inverted, with half of the placenta adherent to its fundus. Hot flannels were applied to the extremities, and a table-spoonful of whiskey and water, (the only cordial that could be procured,) given every few minutes. I removed the ligature that the midwife had applied in the morning, and grasped the placenta and uterus in my hand, gradually pushed it upwards in the axes of the vagina and pelvis until I replaced it in its natural situation, after which I very cautiously detached the placenta in the usual way, and then by briskly rubbing the abdomen with my left hand, while at the same time with the knuckles of my right hand, with the placenta in its grasp, I gently irritated the walls of the uterus, which in about fifteen minutes brought on uterine action, whereby my hand and the placenta were expelled. The operation took up about an hour and a half. No hemorrhage followed. The uterus was well contracted above the pubes. A compress and bandage were applied over the region of the uterus, and the woman's situation in bed made as comfortable as possible; and before we took our leave she was able to express her thankfulness for what was done for her. Cordials and rich broths were given during the night, and sinapisms applied to the soles of the feet. Next morning we had the pleasure of finding her situation much improved. Pulse perceptible, extremities warm, no sickness at stomach, no hemorrhage, uterus well contracted, and had slept a little during the night. From this time she gradually improved until the fourth day, when during the operation of a mild laxative, the uterus became relaxed and in consequence thereof profuse hemorrhage supervened, when Dr. Hannen and myself were hastily sent for. On our arrival

we found the abdomen flaccid and the uterine tumour absent. Our patient was in a faint state from the loss of blood. The plug was introduced and brisk frictions made by the hand over the abdomen. A powder composed of ten grains of finely powdered ergot with two grains of sugar of lead, and the fourth of a grain of opium was given every hour. By the time she had taken the fourth powder the uterus was well contracted, and consequently the flooding entirely ceased. But this second hemorrhage so completely prostrated our patient, that with the utmost difficulty, by the diligent and persevering use of repeated blisters to the extremities, wine, bark, nourishing diet, &c. she recovered in about six weeks or two months, during which time she was attended by Dr. Hannen.

This case strikingly illustrates the efficacy of the ergot in restraining uterine hemorrhage, by inducing permanent contraction of the uterus. It is more that probable, had not the placenta been adherent, the woman would have died of hemorrhage before any assistance could be obtained, as she lived nearly two miles from the city.

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ART. XII. *Case of apparent Superfetation.* By JOSEPH FITHIAN,  
M. D. of Woodbury, N. J.

IT has been a disputed point in physiology, and is to this day, whether superfetation does ever take place in the animal economy.

We know, that nature in the production of our race, generally pursues an undeviating and uniform course, which would lead us to believe, that whether two or more children are born at one birth, they are begotten nearly together, if not at the same time.

Many facts, however, of a curious and interesting nature are from time to time communicated, which appear to be exceptions to this general rule; and, among others, the follow-

ing favours the hypothesis that superfetation does occasionally occur.

Mrs. W. was delivered of a fine healthy child at the full time without any difficulty attending the birth.

After the labour was over, and the assistants were preparing to get her to bed, they discovered something which, from its uncommon appearance, induced them to call my attention to it.

Upon examining this substance, it proved to be a foetus three or four inches long, with its membranes and placenta complete; all of a fresh, healthy appearance, and had undoubtedly been alive and healthy up to the time of the labour.

I have frequently seen abortions at every period of gestation, and from the appearance of this foetus, had I been called to see it without any knowledge of the circumstances, I should without hesitation have pronounced it an abortion at the third or fourth month. It had the large head, small extremities, and general appearance which characterize a foetus at that age.

I have communicated the above fact, as it occurred to me in my practice. I will leave it for others to draw their inferences.

## REVIEW.

*“Letter to the Chairman and Deputy Chairman of the East India Company, on the Cholera of the East Indies. By Sir GILBERT BLANE.”*

[Contained in the Edinburgh Medical and Surgical Journal, for July 1825.]

SIR GILBERT BLANE, may be regarded as one of the most distinguished advocates of the doctrine of contagion as related to the great epidemic diseases. He published in 1819, a book called *Medical Logic*, and as that book contains an elaborate article on the contagiousness of yellow fever, and contagion in general, to which we intend frequently to refer, we shall make some extracts therefrom, in order that our readers may refresh their recollection, with what he has said, previously to publishing this letter on Indian Cholera. The subject of contagion, has long divided the medical profession; and a degree of harshness and asperity, has been carried into the controversies which have arisen out of it, that are far from producing any good effect, either in establishing, or overthrowing the doctrine. The writings even of Noah Webster, on pestilence,\* who as he belonged not to the profession, might be supposed to enter on the inquiry, with a mind more free from bias or passion than other men, are not free from a vein of keen sarcasm, which was perhaps more excusable at the period in which he wrote, than it can be now, that the nature of yellow fever has been freely discussed for thirty years, and heated imaginations have had reasonable time to cool, yet we are sorry still to find that the terms “melancholy delusion,” “extraordinary hallucination,” audacity and gross ignorance, are bandied from side to side, as if such terms which are unworthy of grave philosophers and doctors, could possibly have any effect in settling a question of pure observation

\* Webster on Pestilence.

and experience—to wit, whether yellow fever, plague, and cholera morbus, are contagious or not.

The occurrence of cholera in the East Indies, and its late progress in Western Asia, revives the interest of the subject in a degree, far beyond its ordinary and general magnitude; and as I am tempted to give my opinion on the matter, I hope to do so, with all that temperance and charity, and respect for the opinions of others, which should always come into any proper inquiry, on this important topic. I do not hold all contagionists for bad men, my spirit is in direct opposition to the persecuting temper which gave occasion to these lines of a troubadour:—

“Que non vogli a maudir ne jura ne meutir,  
N’occir, ne avoutrar ne prenre de altrui,  
Ne s’avengear deli suo ennemi,  
Los dison qu’es Vaudes, & los feson morir.”

That is to say, we do not regard all contagionists as monsters, who ought to be hanged or shot, merely for their opinions.

Sir Gilbert Blane informs us in his *Medical Logic*, p. 130, that “there are three sorts of remote causes, which give occasion to fevers, in whatever climate they may arise: one class of these causes is exhalations from the soil, producing intermittent and remittent fevers, which occasionally pass into continued fevers,” &c.

“The second class of occasional causes, is vitiated human effluvia, generated by the living human body, under circumstances of crowding, filth, want of ventilation, and change of apparel, aggravated occasionally by scanty and unwholesome food; as exemplified in the jail, hospital, and ship fevers, and that of the indigent\* part of the population: and all pestilential epidemics seem to have had a similar origin, diversified

\* How can a fever be contagious which you can characterise as that of the indigent part of the population? Are the contagions then respecters of persons! Would our alms-house typhus spare the king, or even Dr. M’Lean himself! The title of the section of the *Medical Logic* from which we quote is on the ambiguity of language.

according to circumstances not always definable and ascertainable." p. 130.

"The third class consists of that disturbance of the system occasioned by fatigue, insolation, intemperance, the privation of food and sleep, sudden alternations of heat and cold, acting either jointly, or singly, in creating fever." p. 132.

"Of these the second only is found to be contagious." p. 132.

"The *first* shall in the course of this discussion be called the endemic; the *second*, the pestilential or malignant epidemic, or typhus icterodes, as it is termed by some of the systematic writers; the *third* shall be called sporadic." p. 136.

We are informed also that the fevers which proceed from long confined human effluvia, seldom *originate* in the West Indies, on account of the warmth of the climate, which causes the inhabitants to live much exposed to free currents of air; "such fevers, therefore, have become epidemic there, *only* in consequence of infection, (contagion,) occasionally *generated* and *imported* by ships under peculiar circumstances of crowding and filth," &c. p. 138.

We think we shall not misrepresent Sir Gilbert, if we announce him as considering all epidemics to be derivable only from human effluvia; and that no endemic or sporadic causes can ever, by any aggravation or increased intensity, become the causes of the epidemic diseases. Such is the idea which we have formed from reading his Medical Logic.

The doctrine which Sir Gilbert lays down in the foregoing extracts, is perhaps not very clearly maintained in the subsequent part of his book; but if we have understood his meaning, it will be evident that the experience of the last seven years in the East, has not enabled him to maintain the whole of his positions as above; which augurs well, of the speedy and entire abandonment of this strange hypothesis in relation to yellow fever, if not to plague itself, and a consequent adoption of more liberal views, concerning public sanitary precautions, and private communion with, and comforting of the sick; in whom the rights of humanity are often dreadfully violated. The successive surrender of other points will.

ultimately, we hope, bring the whole profession into one accord on the subject of contagion.

In his letter to the chairman, &c. Sir Gilbert begins by congratulating them on the "high professional talents of their medical officers in India," and on the industry, zeal, &c. observable in the late report from Madras. The report he speaks of is "On the Epidemic Cholera, as it has appeared in the Territories subject to the Presidency of Fort St. George. Drawn up by order of the government, under the superintendence of the Medical Board, by William Scott, Surgeon, and Secretary to the Board."\*

He proposes to confine himself in his letter to the "question regarding its infectious, (contagious,) nature, and whether, on any occasion, means preventive of its spreading can be available."

His first remark is—

"That those who advocate the doctrine of non-contagion, appear to me to lay too much stress on the circumstance of great numbers escaping the disease, who have been exposed to it by near approach or contact of the sick," and accounts for their escape on the supposition of "variety of predisposition, and original constitution," without the intervention of which preservative circumstances during epidemics, the whole human race must long since have perished by contagious affections.

It is probable that Sir Gilbert alludes in the above remark, to what is said more particularly by Dr. JAMESON, author of the Bengal Report, namely, that the medical observer did not find himself, or his assistants in attendance upon patients of cholera, more liable to be attacked than such as had no communication with the sick. In these cases too, no precaution was taken to avoid infection or contagion; the most unrestricted intercourse with the sufferers was always maintained, often throughout an entire case. Out of a list of between two hundred and fifty and three hundred medical gen-

\* The only full account of this report which has reached us, is contained in the Ed. Med. and Surg. Journal, July, 1825.

tllemen, most of whom saw the disease largely, only three persons were attacked, and one of these only died.\* Now it does not seem probable, that out of near three hundred persons, exposed as the medical gentlemen must have been, all except three should have escaped by means of any peculiar disposition or constitution, such a rule being evidently more applicable to the three than to the three hundred.†

We know Sir Gilbert Blane is not willing that non-contagionists should illustrate their sentiments by reference to the phenomena of small-pox and measles; indeed he has observed that the virus of small-pox, and that of other epidemics, are not amenable to the same law of criticism; but if his *remark* is of any force, it cannot be denied that it should be especially illustrated and enforced, by comparing it with what we know on this head, concerning the variolous contagion. Is it true, that “variety of predisposition and original constitution,” are really the causes which have saved the human race from entire destruction by this enormous scourge? How many unprotected persons, exposed in a small-pox hospital, will escape the contagion of small-pox? What is it, that occasions small-pox to be epidemic this year and sporadic the next; to be absent from a community for many months or years, and then return, to prevail perhaps as many more? Surely something besides any original peculiarity of constitution or predisposition. There never was a truer axiom in medicine than that of WILLIS, in his treatise de Febribus. “It is the fortune of all men, of man only, and of every man once, to be affected with small-pox or measles.”‡

\* Bengal Report by Dr. Jameson, p. 129.

† “In the district of Nuddea, out of a population of 1,300,000, the disease, between June, 1817, and July of the following year, attacked 25,400 persons, of whom 16,500 died, or more than two-thirds.” Bengal Report, p. 172. More than forty-nine-fiftieths of the population here escaped from any operation of the cause of cholera.

‡ “Convenit enim omni, soli, et semel, variolis aut morbillis affici; si forte quispiam in tota vita immunis-degerit, aut alius in hos affectus sæpius inciderit, sunt hæc rara & inusitata naturæ eventa, quæ communi observationi minime derogant quia *ratum sit*, quod nimirum cuncti, & soli homines, sint variolis & morbillis obnoxii, atque unica plaga, iis absolvi soleant.” P. 254.

We say there never *was* a truer axiom, but the case is now altered by the great and blessed discovery of vaccination. If however, we leave the Jennerian discovery out of consideration, who will deny, that by much the largest number, perhaps nine-tenths of the human race, will at some time or other receive the contagion of variola, if exposed to it. No alleged variety of predisposition, &c. can be sufficient, therefore, to explain why all mankind have not long ago perished by measles and small-pox alone, and much more are they insufficient, if we add in the scale a variety of other virulent contagions. There must be some other reason for the escape of the multitude; what that other reason is, we do not pretend to know. We think, however, that the non-contagionists are right in laying much stress on the escape of such numbers of exposed persons.

If cholera really depended on a poisonous human effluvium, of such excessive virulence, as that which has destroyed 6,000,000 persons in the last eight years, we see not how any hope could be left for the safety of mankind, after the formation of such immense fomites, for in this period more than 50,000,000 persons have had the disease. We are informed in Mr. Jameson's Bengal Report, that 2,000,000 persons had assembled at Hurdwara, on the Ganges, where that river breaks through the mountains, to commence its flow in the plains of India. A religious festival had called these multitudes together in 1783. Cholera morbus suddenly broke out amongst them while at their devotions, and in eight days destroyed twenty thousand persons! The pestilence was stayed as soon as the people dispersed after the termination of their ceremonies. It did not extend even to a village only seven miles off!

We doubt not that twenty thousand sick of small-pox, in the midst of a crowded camp of two millions of persons, not protected, would have communicated disease much more extensively, because the animal poisons are more uniformly capable of producing disease in those who are exposed to their influence, than the aerial sort; they are always one, always the same; while the atmospheric sort do not seem to be al-

ways of one constitution. Small-pox contagion is always so; it never, when weaker or more diluted, produces yellow fever or scarlatina. But an atmospheric intemperies, which shall first manifest itself in the production of a most deadly yellow fever, may end in the production of highly marked cases of dysentery, bilious remittent or intermittent fever; and vice versa. This point has been much enlarged on by HUXHAM,\* by Mr. WEBSTER,† and by Dr. JAMESON in the Bengal Report:—for example, the latter represents the whole country, from Saharunpoor down to Patna, as scourged in the year 1816, by an inflammatory bilious remittent. A similar mortality prevailed at the same time in Cutch, Scindh, and other states on the western side of India; it extended even among the cattle, whose carcasses were strewed in vast numbers over the fields. In the end of that year malignant sore throat also prevailed. Mr. Webster has noticed very particularly the occurrence of anginas, as common *precursors* of pestilence. Dr. Jameson says, “instead of acute dysentery, and other inflammatory diseases, usually predominant at this period, (end of 1816,) it was remarked by the medical men, that the only disorders falling under their notice were low fevers, and other disorders of the typhoid type. Of this a remarkable proof was exhibited in the occurrence of infectious malignant sore throat, *a disease previously known only by name in this quarter of the globe.*”‡

If, as Mr. Webster and Huxham justly suppose, these affections may be regarded as prelude of the storm of pestilential miasmata, it will be admitted, that notice of the approach of the dire destroyer was abundantly furnished in India, for the cholera broke out in 1817, raging with terrible intensity, until its causes becoming as it were exhausted and languid in November of that year, permitted the disease to assume the ordinary character of fluxes, remittents, and intermittent fevers, until the epidemic acquired force and vigour enough to recommence its career in the spring of 1818.

\* On Air and Epidemics.

† On Pestilence.

‡ Introduction to Report, 44.

The fact of the gradation of diseases in the introduction of great pestilence, is fully established by Mr. Webster,\* and affords, we think, some among the strongest arguments against the doctrine of contagion, as showing the gradual increase of a morbid condition of the air, and not a change in the nature of human effluvia, which, as we said before, are apparently not so changeable or susceptible of modifications by seasons, temperature, moisture, or any other circumstances. If to these considerations we add, that out of near three hundred medical officers only three sickened, and that forty-nine-fiftieths of the population of Nuddea wholly escaped any efficient operation of the morbid causes, then we think much stress ought to be laid on their exemption, and that Sir Gilbert Blane's remarks has not so much force as it would at first seem to carry with it.

The second remark of Sir Gilbert Blane is, "that whatever weight may be allowed to the above-mentioned argument, it militates much more forcibly against the supposition of aerial influence," because on that supposition "not a single human being could escape disease, inasmuch as every living creature must breathe the external air; whereas, it is easy to conceive, that if the noxious principle resides in the morbid evacuations of the human body, great numbers may never come within their sphere, either by contact or approach."

We have already said, that the supposition in remark the first does not satisfy us, inasmuch as the cause of exemption therein mentioned is found not to operate extensively in the true febrile and other contagions. We prefer to consider what are called in the Medical Logic, *epidemics*, as originating in morbid states of the air, whether produced by exhalations from the soil, or other sources—such as changes in the electric or chemical constitution of the atmosphere, changes which may certainly take place, although they are not appreciable by the instruments which man has heretofore been able to construct for their detection.

\* On Pestilence.

We cannot persuade ourselves to consider them as produced by any *peculiar* virus or effluvia of the living human body, capable of reproducing in other individuals a morbid virus or effluvia, possessing the same specific properties. Indeed, contrary as this may seem to the doctrines inculcated in his Medical Logic, quoted in the beginning of this paper, Sir Gilbert admits it to be partially true, in this letter to the Chairman, &c. "Nothing," says he, "is more clear than that it, (cholera of East Indies,) has arisen on various occasions without owing its existence to contagion, and without communicating it to others." After this admission there was no escape from non-contagionism, except by a resort to the endemic or sporadic causes of fever classified and quoted above from his Medical Logic. He has laid hold, therefore, of that inconsistency. Is it probable, or even possible, that an *epidemic* contagious cholera can be co-existent with an equally prevailing *endemic* non-contagious cholera? for in India, we think it must be admitted, that there is as much ground for believing in the existence of a non-contagious, (or endemic, as Sir Gilbert calls it,) cholera, as of a contagious one, (or epidemic of Sir Gilbert Blane.) The mass of the evidence, both popular and professional, is in favour of the former.

"Ye cannot serve two masters." If you agree with Sir Gilbert, you must either admit two sorts of generally prevailing cholera, one contagious and one not so, which is absurd; or adhere to one sort, which both is, and is not contagious, and therefore still less defensible; for us we have only one sort, which is not contagious.\*

\* Sir Gilbert's remark may be shown to be not conclusive by a relation of what has occurred in this city and district.

During the three last years we have had an almost unexampled proportion of bilious remittent and intermittent fevers in the country round Philadelphia for an extent of many miles, insomuch that whole families have been affected, not only in the outskirts of the city, but in villages and in the open country. At this moment, although 1825 has been very healthy, a majority of the elegant country seats in the environs, are not inhabited, because their owners are terrified by their severe experience in 1823-4. They consider themselves safer in the city, because during

Some of the phenomena observed in the cholera of the East, seem to indicate that the causes operated as a poisonous cloud, (if we may suppose such a thing,) would have done. The poisonous effect has often been as sudden, as we can conceive. Men in perfect health, arrive at a certain spot, and fall down by dozens; some never to rise again, and others to escape barely with life, after excessive pain and torture.

Cases are also related of large bodies of men entering into such a stratum of poisonous air, as at Nagpoor for instance, and also the troops which suffered so terribly on a march from Ganjam through the Northern Circars. In other cases, the pestilential cloud seems to have been wafted into the midst of a stationary assemblage, as at the camp of Marquess Hastings, on the banks of the Sinde.

the whole of this sickly period almost an entire exemption from disease was enjoyed by those families which resided within the limits of the thickly settled parts of the city, as well as in the incorporated districts. In 1823, the distress from sickness was so great among the poorer classes in the suburbs, that committees of citizens were formed, in order to seek out and relieve the distressed. Many thousand persons were sick, many more thousands, however, continued to enjoy excellent health, although the causes of disease were aerial, and must have been respired by all the population indiscriminately. In an epidemic which ranged over so extensive a surface with little regard to elevation or lowness of situation, *all* must have breathed, but a majority escaped. What saved this majority from sickness? if variety of predisposition and original constitution can be supposed of such numbers it may have operated here, for the poison was aerial and not animal. Only one of our physicians died; and Dr. Lawrance's death was the lamented sacrifice to a most ardent and philanthropic exercise of his professional duties. There can be no question of contagion in this epidemic; it is evidently, therefore, far from true, that where all must breathe all must suffer.

This circumstance of the escape of vast numbers within the limits of the city of Philadelphia, cannot be explained by the doctrine of variety of constitution and predisposition alone, since it is impossible to conceive that in such a large population these circumstances could be found so uniformly to operate. This sickness extended all over the country, east, west, north, and south of us, many miles. The causes of it must have been present with us then, but were neutralized or rendered inefficient by means which cannot be ascertained.

It prevailed in situations higher than the city, and was very rife from Tenth to Schuylkill, most of which is higher than many parts of the city proper.

Indeed, since the disease has spread in Arabia and Syria, the Arabs seem to have taken this view of the matter. They denominate it *El Hawa*, the wind!—the storm! because it passes over their tents and villages with the swiftness of the wind: it appears, and vanishes with equal suddenness, to make its rapid attack on other and distant places.

It very rarely remained in much vigour at any place, more than fifteen or twenty days; and sometimes was exhausted in a much shorter time. Armies have fled from the track of the pestilence, and been safe out of the limits of that track.

The army of the Marquess of Hastings was absolutely broken, and routed; it fled from the encampment on *Sinde*, leaving the camp and the road strewn with the dead and dying, and army equipage, until the main body reached the high ground at *Erich*. In less than fifteen days, his excellency's losses amounted to about nine thousand men of all ranks. Another corps, before alluded to, lost an immense number of men, in passing down the *Coromandel Coast*; so that it was crippled, and rather crept, than fled out of range of the pestilential blast, or *El Hawa*.

We do not comprehend how any man can imagine this speedy and simultaneous destruction to be derived from contagion, generated in the living human body, and communicated from individual to individual. It must have been an aerial poison, and all must have breathed it. The majority escaped, inasmuch as the troops with Lord Hastings amounted to eighty thousand, or more men; but only nine thousand died, or only one out of nine. Sir Gilbert Blane's position, that where all must breathe none should escape, is indefensible, we think, on every fair view of this case; for the non-contagiousness is proved by the cessation of the pestilence as soon as the army had changed its quarters from the low ground of *Sinde* to its subsequent high and healthy station at *Erich*, where it arrived with many sick, and consequently with abundance of fomites—if the fomites consisted of sick human bodies, and if it was not personal effluvium, it was a morbidly constituted atmosphere, of which every individual inhaled.

Sir Gilbert Blane's third remark is—"That there does not appear to have been sufficient importance annexed to a circumstance, which, I apprehend, may be considered as a satisfactory *criterion* for distinguishing contagions, or as they are called, epidemic diseases, from those depending on the state of the soil and air, and called endemic. It is this, that if any disorder affecting a whole community, arises from some noxious principle in the soil and air, it must, in the nature of things, attack *simultaneously*, all who are exposed to it: whereas, if it proceeds from any morbid principle, generated in the human body, its attack will be *progressive*, a certain space of time being necessary for it to pass from individual to individual, and still more from one region or community to another."

In addition to this remark, he inquires whether it is reconcilable to reason that cholera should go the whole distance from Bengal to Bombay, in about twelve months, through any other medium than human intercourse, effected by the march of the British armies to the Pindaree war of that period. Nothing, says he, is more "inconceivable or more repugnant to the *plain, good sense*, of the most *ordinary understanding*," than that this cholera should have progressed from "India through Persia, to Syria and Astrachan, by any quality of the air or exhalation from the soil, either following these tracks or generated in them."

In answer to these remarks we shall proceed at some length to show why they do not convince us of the contagious nature of cholera—and to show that it is surprising, (with all the facts that have been presented in numerous private letters, and especially in the official reports of Bengal, Bombay, and Madras,) that Sir Gilbert or any one else, can regard this disease as propagated by such means as poisons passing from individual to individual.

This dreadful scourge, (the epidemic cholera,) commenced its horrible career, in various parts of the Gangetic Delta, in the summer of 1817. In 1818 it had reached as far north as Saharunpore, high up on the Jumna, and had extended south to Cape Cormorin, its westernmost limit being at

Bombay and Surat, its easternmost at Sylhet: in 1819 it attacked the Isles of France and Bourbon, in twenty-one degrees of south latitude: in 1820 it ravaged Siam, Malacca, Java, the Philipine Islands, the southern provinces of China, and Guzzerat in India: in 1821 it is said to have destroyed sixty thousand subjects of the Prince of Oman, round Muscat; it reached Bahrein, Bassora, Bagdad, Bushire, and Shiraz. In 1822 it spread to Ispahan, Teheran, and Tabriz, to Diarbekir and Moussul; and reached in 1823 the island of Amboyna, in one hundred and twenty-seven degrees of east longitude, to Antioch, in longitude thirty-six degrees, and to Astrachan, in latitude forty-six degrees north; having spread in seven years over ninety-one degrees of longitude, from Antioch to Amboyna, and sixty-seven degrees of latitude, from the Isle of Bourbon to Astrachan, destroying in this career, not less than six millions of human beings.

The disease is generally represented as having broken out first at Jessore, eighty or ninety miles north-east from Calcutta, in the middle of August, 1817, but there was a great deal of cholera in the district of Nuddea, west of the Hoogly, as early as May and June. In July it appeared also at Patna, near three hundred miles direct north-east of Calcutta. In the first week of August at Calcutta, at Dacca, and Dinapore; on the seventeenth at Sylhet and Jessore, end of August at Boglipoor and Monghyr; September 15th at Balasore, 17th at Buxar, 18th Chupra and Ghazeepoor, and by the end of November at Rewah and Cuttack.\* The two last places are separated by at least four hundred miles.

The cause of the disease was in a languishing or quiescent state during the winter of 1817. But in the months of April and May, 1818, it acted again with extreme violence, not only in many of its original places, but also at Ougein, Hooshungabad, Nagpore, (with many other places intermediate,) and Ganjam, all these being nearly on a line of six hundred

\* For these statements, see Tytler's and Evan's Letters, and the Bengal, Bombay, and Madras Reports—also a Report by Mr. Moreau de Jonnés.

and forty miles from north-west to south-east on the map. In various other places on a line north and south of six hundred miles from Goruckpore to Visgapatam, the disease was also to be met with at the same periods of 1818.

These facts, which are detailed in the various reports, are we think, irreconcilable with any theory of progression by contagion communicated from individual to individual.

Sir Gilbert Blane's difficulty lies in its progress from Jessore to Bombay, in so short a period as twelve months. This distance is, on the map, about eleven hundred miles; now if we can show that other epidemics, manifestly not contagious, are found to travel over tracts equally extensive, at a more deliberate or a more rapid rate, we ought then to admit that Sir Gilbert's argument is of no force in proof of the contagion of cholera, for by the method of exclusion in philosophizing, if one *may* travel fast or slow without contagion, another *may* be supposed to do so, at least until its contagious principle of progression is fully demonstrated. To use his own language, "the plain good sense of the most ordinary understanding" must agree with us in this. There is a speciousness in his rule for distinguishing epidemics from endemic diseases; but a little consideration will show that there are two diseases, which under that criterion, (simultaneousness of attack,) ought to be called endemic, more than any others that we are acquainted with. These are, 1st. Influenza, or epidemic catarrh; and 2d. this identical epidemic cholera. The latter has most commonly attacked the population en masse, and not unfrequently, in ten or fifteen days is gone, leaving no traces but the prostrate corpses of its victims, and the tears of their friends and relations. The former, is equally characterised by the simultaneousness of its attack. In this country, it most generally commences in the north-eastern districts, and in its progress to the southern states, attacks the population so suddenly and simultaneously, that in five or six days, the majority of persons in any small district attacked, are found to be affected with some of its symptoms; and, this not in cities only, but in villages, and in the open country. Such an epidemic presents, therefore, a

fair case of what by Sir Gilbert's criterion should be called an endemic catarrh; here is simultaneousness, but this influenza has not only commenced in the north-eastern sections of our country, it has *progressed* regularly down the atlantic states, in a south-western direction, sometimes faster, sometimes slower, a feature, which if we use Sir Gilbert's criterion, must exclude it from the class of endemics, and bring it under the head of epidemic contagious affections. It has been known to pass from us, over to the West India islands, and on some occasions it has travelled over the globe; as in the universal catarrh of 1733, which commenced here in autumn. We had an influenza here in 1781, in 1782 it came into Europe from Asia, and Mr. Webster suggests, that it passed across this continent to China and Kamtchatka, and so on, over Asia to Europe; it travelled therefore, (if the suggestion be just,) about twenty-five thousand miles in twelve months, which no man or ship has ever done. The progress of different influenzas is very different; for example—on the 11th of August, 1807, one-half the inhabitants of New York, were supposed to be sick, or lately to have been sick with influenza. It progressed as far as Raleigh, N. C. by the middle of October. The distance from New York to Raleigh, on a line, is three hundred and fifty miles. The epidemic moved therefore about seven or eight miles per day.\* Cholera progressed from Jessore to Bombay in twelve months; say one thousand one hundred miles, at the rate of about three miles per day. It went from Allahabad to Etawah, at five miles a day; from Etawah to Agra at about two miles. The typhoid pneumonia, epidemic in the United States, from 1810 to 1816, may be cited as an example of very slow movement. It began its progress southwardly in 1810, and reached Philadelphia in the winter of 1812–13. It raged in the midland parts of Virginia in 1814, and reached many places in North Carolina in 1815, proceeding thence to scourge South Carolina and Georgia in 1816. This was not a contagious disease, if we may rely on the testimony of our own senses, and the opinions of physicians, and people. Its progress from Boston to Columbia, capital of

\* Ozanani's recent work on Epidemics, contains abundant confirmations of these positions, as to the rate of movement of influenza.

South Carolina, took up six years, which gives a rate of less than one hundred and fifteen miles per annum, or less than one-third of a mile per day. While cholera goes two, three, four, five, or six miles, our influenza of 1807, seven or eight miles, and that of 1781-2, sixty-eight miles per day. It is therefore manifest, we think, that neither the simultaneousness of attack, or the rapidity or slowness of progression, can afford a sure criterion for distinguishing between the different classes made out, either in the Letter, or the Medical Logic.

Inasmuch as Sir Gilbert's letter is written on account of the publication of the Madras Report, we feel bound to inquire whether the criterion derives any stability from the facts contained in that excellent paper? Does that report show any example of simultaneous attack, and is the movement always progressive, as from individual to individual?

Independently of the horrible havoc at Hurdwara, in 1783, already mentioned, where twenty thousand perished in eight days—of the destruction of seven hundred men of Colonel Pearce's corps at Itchapore, (1781,) in eight days—and the route and overthrow of Lord Hastings's army on the banks of Sinde, where nine thousand perished in about two weeks, we have in the Bengal and Madras Reports many examples of great suddenness and simultaneousness, as in the following case:—

“The first spot in the northern and central region of the peninsula, at which it is represented to have appeared, is Nagpoor, and the neighbouring villages; where it was observed about the middle of May, 1818, when it was general and fatal among the inhabitants; but no case appeared among the troops until the 26th or 27th of May, when three or four men of the depot corps were attacked and died. On the 30th of May a large detachment of Bengal and Madras troops arrived at Nagpoor from the siege of Chandah, and took possession of certain huts at the foot of the Sitabuldee hills, which they had formerly occupied. Though previously in good health, they had scarcely taken possession of their quarters, when it appeared in a violent manner among the Bengal troops and their followers. Of the Madras troops only one individual was *this day* affected, but on the 31st.

(next day,) it appeared with great violence and fatality. On the 1st of June the attacks were very numerous, but the deaths proportionally fewer; on the 2nd it declined, and after the 10th rarely occurred. In a detachment of the force recently employed at Chandah, which was left at Hingumghat, fifty miles south of Nagpoor, it appeared at the same time and followed the same course.”\*

Now we would ask if you can want a more simultaneous attack to prove by the criterion an endemic origin, than the one just quoted? Can you attribute to a virus passing from individual to individual, such a scene of sudden death and consternation as that of Hurdawara, of Itchapore, and the Sitabuldee huts? Can you account for the appearance of contagions of such unparalleled virulence, in the populous northern and central region of India, in latitude twenty-one degrees, where from the doctrines of the Medical Logic, the heat of the climate implies free currents of air, and well ventilated habitations, which prevent the generation of contagious epidemics. It had prevailed as far back as November, 1817, (i. e. four months before,) at Rewah, two hundred and sixty miles north-east, and in October at Cuttack, four hundred and fifty miles east of Nagpoor. Where was this poisonous human effluvium generated, how was it conveyed harmless through a dense Hindoo population, secreted from November to May, as in a box of Pandora, to be scattered with such horrible and hasty destruction on the distant towns and villages, not secure in their remoteness! The Arabs may, indeed, with great propriety call it *El Hawa*, the wind! the blast that passes!

Cholera appeared at Punderpoor “on the 14th July, (1818,) while the place was crowded with strangers, assembled for the celebration of a great festival; and after producing great mortality, and extending itself to the troops in the vicinity, it declined about the 24th of the month,” (ten days.)‡

\* Vide *Edinburgh Medical and Surgical Journal*, July 1825.

† “In Nagpoor the Medical Staff remained for several days, night and day, in the hospitals, and yet all escaped.”—*Bengal Report*, 129.

‡ Vide *Madras Report*, ut supra.

REHMAN, in the Edinburgh Medical and Surgical Journal, informs us, that in a few days of September, sixteen thousand perished in Shiraz.

Mr. BARKER, an English consul in Syria, in writing an account of its progress in that country, has a very remarkable instance of simultaneousness of attack.

“On the 6th inst. (July, 1823,) while I was at Suedia, between Antiochia and Latachia, a report prevailed of one or two doubtful cases of cholera morbus. On the 7th and 8th, nothing more occurred; on the 9th, *precisely at twelve o'clock*, two of my workmen were attacked, and at the *same instant*, nearly twenty fine robust men, in the prime of life, who were ploughing or reaping in the fields, were laid prostrate as if they had been shot through the heart. Most of them died soon after the setting sun, and none survived long enough to see it rise again.”\*

Does this look like contagion passing from man to man, or like the Arab's El Hawa? We are ready to grant that the disease followed,† accompanied,‡ and preceded§ the march of the troops in India, but whether it did one or the other ought really not to weigh a feather in the scale of judgment for contagion, since the simultaneousness of attacks, and many other circumstances wholly irreconcilable with any such theory, however unlimited, gives us the best example after influenza of the endemic origin, (as Sir Gilbert's criterion would call it,) if this great epidemic, whose coincident extension to Bombay|| during the Pindaree war, affords no real or substantial argument of its contagious extension. An epidemic may attack the people slowly, and in succession, without being contagious, but when it attacks simultaneously, the cases cannot depend on contagion, generated in, and communicated from, individual to individual. If fifty thousand persons should be seized with small-pox in the same hour in this city,

\* Vide M'Lean's Evils of Quarantine, p. 435.

† At Hansi.

‡ Jaulnah.

§ Nagpoor.

|| It extended to Cape Cormorin, thirteen hundred miles, in the same period, without having the excuse of any great military movements, in explanation thereof.

who would attribute it to human contagion, or who would deny it to be a contagious disease.

Cover all the large towns in your map of Hindostan with bits of paper or grains of corn, (we mean such as suffered in 1817-18,) and you will at a glance perceive, from the scattered and wide extended range of the malady, that it is far easier to explain its progress by reference to some atmospheric intemperies, or exhalations from the soil, occurring in different places at the same time, or successively, than by any one specific human virus or effluvium, communicated only from man to man. Its march across the peninsula from west to east, is any thing but a narrow track—e. g. In July, 1818, it was at Hansi, not far from Delhi, and at the same time at Jalna, six hundred miles south of Hansi, with many intermediate places.

If its tract down the Coromandel side was narrow, it was because the low grounds of the coast, are more favourable to its operation, than the high mountainous interior, which served to keep it along the shores of the Bay of Bengal.

The fourth remark of Sir Gilbert Blane, is—

“That those who have argued against the existence of contagion from the impossibility of tracing it, except at considerable distances, do not seem to be aware that the like argument might be adduced against the contagion of small-pox and measles, which, as every one knows, are frequently caught under circumstances of time and place, which would be held decisive against their being contagious, were it not that this is fully established by other facts.”

We freely confess, that this remark has occasioned us more difficulty and embarrassment than any other in his whole letter. We have no doubt of the contagion of small-pox, and that it has appeared in districts which afforded no apparent sources of contagion; but if it has afterwards spread by means of contagion, if persons exposed to it have taken it within a given time, which is nearly the same in all cases, then we are at liberty to suppose the questionable case, derived from contagion, or not, according to the evidence. We are always

free to believe that small-pox *may* possibly arise, independent of any communicated human contagion. Small-pox is comparatively a new disease; how did it originate? Can any man be found, who is of opinion, that every particle of small-pox virus now on earth, is to be traced, even in imagination, back to some Arab freebooter, during the war of the elephant? He would be as reasonable, to maintain that every particle of leaven on earth, is a sort of palingenesis of the Egyptian leaven of the epoch of Pharaoh.

Variola is *contagious*, because whether it is spreading epidemically or sporadically, it will give small-pox to every unprotected person immediately exposed to it.

Cholera is not contagious, because it will not give cholera to every one exposed, nor even to one-half of them.

Small-pox does become epidemic. Willis has observed, (and Moore has ridiculed him for it,\*) “that a particular constitution of the air, has great effect in producing small-pox and rendering it epidemic or popular.” “Secundo, peculiaris quaedam aeris dispositio variolas insigniter producit: hinc sæpissime *popularis* evadit, ac per totas regiones, urbes, vicosque desævit,” &c.†

The contagionists admit that a clean ship, with a healthy crew, may leave a healthy port, and in the progress of a long voyage may generate yellow fever, which, if imported to the West Indies, becomes contagious epidemic, affecting such as are exposed. No contagion originated this fever—it was crowding, filth, &c. Why not admit that small-pox may also arise, independently of contagion. You are not bound to deny this possibility, nor can you do so, without going back for its origin to the immaculate body of Adam, who did not issue from the hands of his Maker, a foul and bloated spectacle of the most disgusting of all diseases. Admit then the possibility of variola arising without previous contagion, yet always possessed of this quality, and Sir Gilbert’s objection falls to the ground, and our embarrassment with it.

\* Moore’s History of Small-Pox.

† Willis de Febribus.—Huxham in his preface has the same remark. Vide Huxham on Air and Epidemics.

Let us not be misunderstood, we repeat, therefore, that we believe in the contagion of small-pox; reject the doctrine in yellow fever; and are doubtful of it in plague;\* holding us ready for the arguments pro and con, without any decided bias for either side.

Sir Gilbert Blane's fifth remark is—

“That both parties in this controversy have truth on their side. Nothing I think can be more clear, from the very luminous history of this disease, as given in the Madras Report, than that it has arisen on various occasions, without owing its existence to contagion, and without communicating it to others, as exemplified in the cases of a very limited number of individuals, unconnected and uninfluenced by each other, in which circumstances, after a partial prevalence, the disease has disappeared without spreading, as stated in several passages of the report, while it is equally manifest, from other parts of the narrative, that the disease was certainly contagious; nor is there any thing contradictory in this, for it is perfectly consistent, abstractedly considered, that the concurrence of two causes may be necessary to the production of one effect; nor is it dissonant to reason and experience, when brought into comparison with analogous cases, for it is fully ascertained with regard to the typhus fever of Europe, and the yellow fever of the West Indies, that though they sometimes appear in a sporadic and uninfectious form, they

\* The plague existed at Noja, in Naples, from Nov. 1815, to June, 1816. The population of Noja was 5300 souls. 1474 persons, belonging to 291 families, either had, or were suspected of having the plague, during the period above mentioned. Of these 716 died, to wit:—In Nov. 8—Dec. 36—Jan. 237—Feb. 157—March, 144—April, 52—May, 81—June, 6—Total, 716. Of the remaining 758 persons, 546 were only held suspected, and 212 really affected with, and cured of the disease. During the greatest part of the period, Noja was enclosed by a sanitary cordon, who inflicted summary punishment by death, on every one who attempted to leave the city contrary to authority. Didonna was shot for throwing over a pack of cards beyond the cordon. Another man was shot down, as if he had been a wild beast, because he ventured beyond a prescribed line. The history of its introduction, its extension, and cessation, afford many arguments against the contagious nature of the plague of Noja. Vide *Storia della Peste di Noja*, by Morea.

do also, under certain other circumstances, assume a form decidedly contagious. These circumstances are chiefly *vicissitudes of weather*, increased susceptibility created by *fatigue*, privations, crowded accommodations, want of cleanliness, and deficient ventilation, which *add* concentration and virulence to the venomous principle," &c.

We ask as a favour of the reader, to turn to those passages of Sir Gilbert's Medical Logic, which are quoted in the commencement of this paper, in order to see how much of the ground therein taken up, is abandoned in this, his fifth remark.

He remarks at p. 136 of his Medical Logic, that all the three sorts\* "are in vague and vulgar language styled the yellow fever," but he argues at great length, to prove that they are not identical, that the yellow fever is an imported disease, generated in ships, and propagated by human effluvia; and that the endemic and sporadic forms only resemble it slightly, (see p. 169, 137, 133.) and therefore differ from it; while in this fifth remark it is said to be sometimes not contagious, and at other times highly so; admitting them to be identical.

We may now remark, that cholera of Asia either is, or is not an epidemic, according to his *criterion*. If epidemic, it arises from vitiated human effluvia, produced by crowding, filth, &c. and must be contagious; otherwise it is endemic, or sporadic, and not contagious. Both parties are right, says Sir Gilbert, and there are two causes abstractedly considered, for the one effect, cholera; we cannot in conscience, admit these two causes, that is to say, we cannot find one cause to produce in you, a yellow fever which is decidedly contagious; and another to produce in me, a yellow fever which is decidedly not contagious. Gold is heavy, ductile, maleable, &c. if you take away the ductility, gravity, or maleability, it ceases to be gold, but is become something else which is not gold. So yellow fever say you, is a fever attended with certain gastric symptoms, and a contagious quality; if you take away these gastric symptoms and contagious quality, it ceases to be yel-

low fever, and becomes something else ; you destroy its identity as yellow fever, and make of it catarrh, or what you will. Small-pox generally arises from contagion of human beings; it once arose, independent of human contagion, but rise as it may, it never fails to be a contagious disease; a small-pox, which is not contagious in some of its stages, is not variola, but something else; it loses its character—its identity—so also of measles.

We cannot agree therefore that in Sir Gilbert's sense of the word, yellow fever is ever epidemic; or cholera either, because it is sometimes not contagious by his own admission; and therefore, *may* be always held so, until the contrary is *proved*.

If cholera, yellow fever, and typhus, may *ever* arise without contagion, or contagiousness, they may *always* do so. If in Nagpoor, Havannah, and Dublin, why not in Bombay, Philadelphia, or London! If you extinguish cholera at Cuttack, it may break forth at Ougein, or Ganjam. If you do not import yellow fever from the West Indies, it may arise at a wharf in Philadelphia, and if you quell typhus in one street, it may break out in other. Of what value is a piquet guard when the enemy is in your camp. The sanitary cordon is the picquet guard, he cannot keep the enemy out, but he confines him within your city to rifle your houses, and murder your wives and children.

The miasmatic causes of disease, are better classed by Dr. Broussais, than by Sir Gilbert Blane; and since we are so far engaged in the subject of contagion, we think it impossible to do better than translate from a new work, a clear and lucid statement of that author's doctrine on this point. He considers them susceptible of a three-fold division.

“1st. Those derived from the decomposition of dead organic substances, marshes, low grounds on the sea shore, burial grounds, privies, and places where dead bodies undergo decomposition, imperfectly excluded from the air.

“2d. Those produced by the assemblage of many animals, whether sick or in health, prisons, hospitals, besieged towns. and ships on the high seas.

“3d. Sources consisting of sick individuals, who communicate to healthy persons, the diseases they themselves are suffering. Hence, plague, yellow fever, jail and hospital fevers, which are found in our climates, depended on the two first named causes, since they are never propagated beyond the place where they are produced. Small-pox and measles belong to the last, since they communicate themselves from person to person, in all imaginable atmospheric conditions.”\*

Now, whether we choose to consider cholera as originating from the first or second, we have no difficulty in finding cases explainable, on either hypothesis. The devastation effected in the army of the Marquess Hastings is an example of the first, and the occurrences at Hurdwara of the second class of efficient causes. In the latter, the disease originated in the midst of an immense collection of human beings, (two millions,) and ceased with their dispersion, not extending even to a village seven miles distant. As twenty thousand of these persons died, it may be supposed, that at least forty thousand were affected with it. If contagious, here were forty thousand radiating foci of contagion. How can you explain the fact that these foci ceased suddenly to shoot forth the arrows of death? it is impossible to do so, on the supposition of contagion.

Sir Gilbert Blane’s doctrine of contagion of cholera, is as fairly to be refuted in his *Medical Logic*, as in his *Letter to the chairman, &c. &c.* This is the reason why we dwell so much on yellow fever in these remarks. A great deal has been said and written on the matter already, but enough will not have been said, until all men are agreed to one sentiment or the other; for we are firmly persuaded, that all men are ready to be convinced, as soon as the proofs that can convince them shall be offered. We hope to be considered as not too tedious if we make a few remarks here, which, though they may not convince others, are, we think, sufficient to warrant our own confidence in the doctrine of non-contagionism of yellow fever. We have had that disease in this city,

\* Vide *Catechisme de la Médecine Physiologique*, p. 60.

(Philadelphia,) since 1695, at various periods. The first quarantine law was passed in the reign of William III. A. D. 1700. It provided that no unhealthy vessel, coming from an unhealthy or sickly port, should come nearer than one mile to any port, (within this jurisdiction,) without bills of health; nor shall land any sick passenger, under certain penalties. This law being found of no avail, a second act was passed in the 16th of Geo. II. 1742, in consequence of which our present Lazaretto station was procured on Province Island. A third act was passed Jan. 22, 1774, which placed the establishment on an excellent footing, though it has been improving since that time. Nevertheless, we have had yellow fever in the following years, to wit:—In 1695, 1746, 1747, 1748, 1749, 1751, 1762, 1793, 1797, 1798, 1799, 1802, 1803, 1805, 1818, 1819, 1820, and a few cases in 1825.

This table shows that we have had the disease so often since 1742, that we are at last warranted in concluding, that quarantines cannot keep us free from it always; if it argues more with the non-contagionists, well—if it argues no more with the contagionists, we cannot help it.

But whether it weighs with the contagionists or not, there is proof enough that something more than a human effluvium, or filth, or famine, or want of ventilation, is necessary to the production of yellow fever here, since it never occurs here, unless the temperature at certain seasons be above an ascertained degree. What peculiarity of soil, or atmospheric intemperies, what nearness to, or remoteness from, tide-water, what exhalations from decomposing organized matters, are the requisite accompaniments of 79° of temperature, and necessary to the development of yellow fever here, we cannot tell: we only know that Philadelphia has suffered when the mean of June and July in any year has been above 79°, and that it has never suffered when it has been below that grade, except in 1802, slightly.

We shall take the liberty of republishing Mr. EVANS's table of comparative temperature, which is contained in the *Eclectic Repertory*, vol. 7, p. 428.

Years.	Mean heat, June, 3 P. M.	July, 3 P. M.	Average, June, July, 3 P. M.	Mean, Aug- 3 P. M.	Mean, 3 months, 3 P. M.	
1793	79.7	84.3	82.	82.7	82.2	{ Great mortality by yellow fever, 1000 died in Aug. Sept. and Oct.
1794	75.6	80.4	78.	81.7	79.2	
1795	75.	82.2	78.6	80.3	79.2	Do. do. in New York or Norfolk.
1796	76.5	81.5	79.	80.3	79.4	No alarm here.
1797	79.	84.2	81.6	79.	80.7	Yellow fever here, 1250 died in 3 months.
1798	77.	82.	79.5	86.5	81.8	Great yellow fever here, 3500 died in 3 months.
1799	77.	84.	80.5	82.	81.	Yellow fever here, 1000 died in 3 months.
1800	75.	78.	76.5	78.	77.	No alarm here. Yellow fever in Baltimore.
1801	76.	80.	78.	77.	77.7	No alarm here.
1802	75.7	78.	76.3	78.	77.2	{ Yellow fever spread a little, perhaps 200 adults died of it.
1803	76.9	81.8	79.3	79.4	79.3	Yellow fever slightly.
1804	71.	78.	74.5	75.	74.7	No alarm here.
1805	75.	83.	79.	81.	79.7	{ Yellow fever began about 1st Sept. very mor- tal in Southwark, and little out of it.
1806	78.1	78.7	78.4	72.1	76.3	{ All these 12 years were cooler than 79° on a mean of June and July, at 3 P. M. and there was no alarm of yellow fever in Phi- ladelphia in any one of them, I believe, or none that spread and continued.
1807	71.6	77.9	74.7	75.2	74.9	
1808	75.5	78.8	77.1	76.5	76.9	
1809	73.7	75.1	74.4	76.7	75.1	
1810	74.2	75.4	74.8	75.4	75.	
1811	74.4	80.1	77.2	75.3	76.6	
1812	75.8	77.4	75.6	74.2	75.1	
1813	75.3	76.7	76.	77.3	76.4	
1814	70.1	73.2	72.6	76.7	74.	
1815	74.2	81.8	78.	77.3	77.7	
1816	72.5	73.5	73.	76.5	74.1	{ These years were published by Mr. Evans in 1819, just previous to his death.
1817	73.2	78.	75.6	77.3	76.3	
1818	78.	80.7	79.2			
1819	79.4	80.8	79.4			
1820 mean heat of June was 78.6						{ 123 cases occurred, } Dr. Jackson's Paper.*
July 82.1						
Aug. 79.8						
Two cases of yellow fever occurred in 1818.						
Twenty-four cases occurred in 1819.						

Mr. Evans has thus made it clear that a temperature of 79° is requisite to any considerable extension of the causes of yellow fever among us. It is manifest, however, that something else beside heat is requisite. The disease prevails at Vera Cruz, but never at Acapulco. Is it conceivable, that the galleons which come there off such long voyages, with many passengers, can never generate contagion of yellow fever, while vessels arriving at Vera Cruz, on the opposite shore, are never without it almost? The fever at Guayaquil was said to be imported by the galleons. If contagious, how is it possible to account for the fact that yellow fever has not desolated more places than are comprised in Sir Gilbert's list in his Medical Logic? We will give this list, imperfect as it is, but shall preserve the chronological series, which he has neglected.

\* Vide Dr. S. Jackson on Yellow Fever, in Chapman's Journal, Vol. I. p. 319.

*Yellow Fever prevailed in*

1647	at Barbadoes,	1746	at Cadiz,
1686	Martinique,	1748	Charleston,
1687	Olinda in Brazil,	1751	Philadelphia.
1693	Boston,	1762	Philadelphia.
1695	{ Philadelphia,	1764	Cadiz,
	{ Charleston,	1793	{ Grenada,
	{ Barbadoes,		{ Philadelphia.
1702	New York,	1794	Charleston,
1723	Lisbon,	1795	New York,
1729	Carthagená,	1800	Cadiz,
1732	{ Cadiz,	1801	Cadiz,
	{ Charleston,	1803	Malaga,
1733	Cadiz,	1804	{ Cadiz,
1734	Charleston,		{ Gibraltar,
1740	Guayaquil,	1810	Gibraltar,
1741	Malaga,	1813	{ Gibraltar,
1744	Cadiz,		{ Cadiz,
1755	Charleston,	1815	Cadiz.

This account takes no notice of our great fevers here subsequent to 1793, nor does it notice the disease at Baltimore and Norfolk, &c. It has no pretensions to great accuracy, but imperfect as it is, does it not show the impossibility of explaining, why such a violent contagion as that of yellow fever, did not attack more than twelve places, during more than one hundred and fifty years of an active and enterprising commerce, when quarantines had no existence in many of these places; and when some of these places too, are separated by such great distances as from Olinda in Brazil, eight degrees south latitude, to Boston in forty-two degrees north; Guayaquil on the eastern shore of the great Pacific, and Lisbon on the eastern shore of the north Atlantic Ocean.

But the doctrine of non-contagion of yellow fever is unquestionably gaining ground; few professional inhabitants of places where it has raged in America, consider it to be contagious, or dependent on any other than local causes. This was the case with the majority of physicians, whose opinions have been expressed in documents soon to be published by Dr. CHERVIN, who visited the United States a few years

since, for the purpose of making inquiries into the contagion or non-contagion of yellow fever.

As to the contagion of Cholera, it is rejected by the majority of practitioners in Bengal and Madras. The people generally rejected it, and it was only at Bombay, that any thing like a division of sentiment was discoverable in the profession.\*

We are for ourselves persuaded, that no good can be the result of a general belief in the contagion of cholera. The coarse and vulgar invectives, with which the champions of opposite sentiments, are wont to assail each others doctrines through their persons, can never operate beneficially in the settlement of this important question, but only keep alive the unhappy personal discussions which are so disgraceful to the humane and philosophical profession, which they are perhaps intended to illustrate and advance. The question is of vital

\* Dr. Jameson says, "in no quarter of India, during the time in which it was so sadly scourged by the disorder, did its infectious nature form any part of the popular belief," p. 123. Again. "The whole body of the medical officers in Bengal, who have had an opportunity of seeing and remarking on the disease, without a dissenting voice, concur in declaring that it is not contagious." p. 124. Even let us admit that three-fourths of them are of this opinion, and it will afford a strong argument against their opponents theory.

There is a letter in the *Med. Chir. Trans.* vol. xii. p. 359, from J. Cormick, Esq. It is dated Tabriz, Oct. 22d, 1822. In it he says "The spasmodic cholera of India reached last year as far as Shiraz; and this year we have had it here, and in almost every other city of Persia.

"The disease first began in that part of the city which is most low, filthy, and crowded with poor inhabitants, and advanced from quarter to quarter of it, finishing its ravages in one before it commenced them in another. It was most destructive in the houses which were low, and possessed most inhabitants. In no case did I see a patient abandoned by his friends, under the idea that this epidemic was contagious. This idea indeed seems to be pretty generally abandoned." Mr. C. informs us that the family of the prince left the city about the close of the epidemic, but they carried the epidemic along with them wherever they went; but though from four to six of his followers were attacked every day, they never communicated the disease to any of the villages through which they passed or in which they slept; it followed them in this manner for about ten days.

importance; if the non-contagionists are right, then the sooner we get rid of the burden which is falsely imposed on commerce, and the inhumanity which is exercised towards sick individuals, the better; if the contagionists are in the right, then no sanitary cruelty, no hanging, shooting, or stabbing, or imposition of fines on the unfortunate persons who are inclosed within infected limits, is unpardonable, but on the contrary must be regarded as a wholesome severity, a mere measure of justice to the community whose health is endangered.

The question is capable of being settled. Neither the obloquy which is poured out with unsparing hand on Dr. M'Lean, nor the biting sarcasm of his retorts, can prevent the establishment of the truth: "*Magna est veritas & prevalebit.*" No torrents of invective can tear up its firm basis, for like the house upon a rock, it shall stand when the storm of passion is exhausted in vain and fruitless assaults.

Sir Gilbert Blane closes his letter with some remarks on the "great point of practical importance, whether from what we know of this dreadful disease, any hope can be entertained of employing practicable and available means of arresting its ineffable horrors." The details under this head are by no means of a decided character, especially in relation to cholera in Persia. He says, "though cholera has visited Shiraz and other cities of Persia, Ispahan has escaped, in consequence, it is believed, of the governor having prohibited the usual caravans to go there, and ordered them to Yezsd, where it broke out in common with other towns."

A caravan cannot be necessary even in Persia to the conveyance of a contagious disease; and if it were so, who can believe in a Persian police sufficiently vigilant to keep up an efficacious non-intercourse—in fact, we are informed by Mr. Barker, that it broke out at Ispahan the next summer, and was only prevented from going there at first by a change in the weather. It is not wonderful that the disease travelled up the Euphrates and over to Syria by the route of the largest cities, nor that it should have crossed Persia and Georgia in the same manner. It is only in large cities that epidemics find hecatombs

of victims, to make their course remarkable, and the few Arabs and Persians, and Turks, who have perished in villages and deserts, are not, on account of their insignificance, enumerated. It is very evident that the causes of the disease then have not been confined to a narrow track, they have operated on an extended surface from Teheram to Antioch, and from Bassora to Astrachan, in this, it has only acted as in India it had done before. If the people can only be saved by a Persian police, heaven help them, for they will have need of help; but they know that flight alone can certainly save them, when the blast descends among their cities and tents, it is El Hawa! they flee from its track, and are saved.

From what has been said in these remarks, it will be evident that Sir Gilbert Blane has abandoned some of the ground which he took in the Medical Logic. He admits that both parties are right in this controversy, that the disease has sometimes originated independently, and has been propagated independently of any contagious property. The difficulty which he labours under, in attempting to reconcile the phenomena of the contagious and non-contagious forms of yellow fever and cholera morbus, is very manifest in a perusal of his observations on those subjects already quoted.

We think he has not proved the fallacy of the argument for non-contagion, derived from the exemption of the majority of persons.

The argument, or rather the criterion of simultaneousness, or progressiveness, is assuredly not satisfactory, when compared with the facts we have stated under his third head, any more than his supposition, that where all must breathe, all must suffer if the morbid cause is aerial.

We are not convinced by his fourth remark, to wit—that as small-pox is occasionally met with, in circumstances where no trace of its origin by contagion can be found, so we may argue that cholera has arisen also by contagion, even in cases where no trace of it has been discoverable.

His fifth remark, that cholera is both contagious and not contagious, under certain circumstances, is in contradiction to

his positions in the *Medical Logic*, and inconsistent with all the analogies which we can find in small-pox, measles, &c.

We shall take our leave of this subject now, with the expression of a sincere respect for the services rendered by Sir Gilbert Blane to the cause of humanity, in his professional career. We cannot agree with him on the subject of contagion; but we have the highest respect for the laudable zeal, and the active philanthropy which he has always manifested in the establishment of what he has considered to be the truth on the subject of contagious epidemics.

We hope to see no sanitary cordon adopted with regard to cholera. The tendency of Sir Gilbert Blane's remarks, is evidently of a nature to cause such to be adopted, provided, in the course of the next summer, Europe should begin to feel the embraces of this monster, whose progress, like the fabled snake, may girdle the world.

## QUARTERLY PERISCOPE.

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### EUROPEAN INTELLIGENCE.

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#### THERAPEUTICS.

*Successful Mercurial Practice.*—Dr. Latham in his interesting work on the late dreadful disease in the Milbank penitentiary in England, thus speaks of this invaluable medicine:—

“We first made trial of this remedy in those cases which our experience had brought us to regard with the greatest apprehension, cases, (if I may so say,) of mere passive diarrhœa, where there was no excitement of the circulation, where there was little pain, and little of morbid quality in the evacuations, but where the evacuations were enormously frequent, and hitherto absolutely incontrollable. In these cases all medical expedients had failed, and we were now compelled to content ourselves with such temporary relief, and such short intervals of ease, as opium, administered in draughts, or clysters, or in cataplasms, was able to procure. In our trial of mercury for these cases, we proceeded thus:—Equal quantities of hydrargyr. c. cretâ and pulv. ipec. comp. were made into pills; each pill consisted of five grains, two grains and a half of each ingredient, and one of them was administered, three times a day, to about twenty patients. Still there was no abatement of the diarrhœa. They were administered four times a day, and still the diarrhœa continued. They were given five times a day; when, upon our next visit to the penitentiary, we found, among those who had taken mercury, one female in a profuse salivation, and the diarrhœa completely arrested in her, and in her alone. This poor creature had formerly had scorbutic spots upon the skin, at the same time that she suffered a flux of the bowels. The scorbutic spots had disappeared altogether; the flux had subsided, but returned;

and that form of it, which has been described, had now brought her life into imminent hazard.

“In this instance, the salutary effect of mercury was unquestionable; and the condition of its success seemed to be, that it had procured salivation. We proceeded, therefore, more boldly in the use of it, still giving it in the same form, and in combination with Dover’s powder. We increased the dose to those who already took it; and, as they became salivated in succession, they were all freed from the symptoms of their disorder. We subjected more and more of the prisoners to the same treatment, watching them carefully in the mean time, for the sake of still more confidently ascertaining the precise condition which was essential to the success of the remedy. This we uniformly found to be the production of salivation.

“Be it remembered, that these cases, upon which the salutary effect of mercury was first proved, were those which occasioned us the greatest apprehension. Several of the patients were so feeble and emaciated, so pale and faded in their aspects, that while, on the one hand, we were feeling our way with the mildest preparation of mercury for the purpose of curing their disease, we were, on the other hand, administering wine and cordials for the purpose of upholding their existence.

“The success of mercury, under these unpromising circumstances, led first to the more general, and, finally, to the universal employment of it: we resorted to it in every case of flux, where the remedies hitherto used had not satisfied our expectation. In short, we resorted to it in every case without exception.

“But, as it became more and more obvious that salivation was the condition of its success, there was no reason for restricting its use to one preparation only. Yet, at the same time, it was not enough that salivation should be procured in any way, gradually or at once, quickly or slowly.

“Experience taught us, that its curative effect depended, in some degree, upon the manner in which salivation was brought about. Hence, a choice and a discretion were to be exercised upon the kind of preparation, the quantity and frequency of the dose, and its combination with other remedies.

“Where the flux was attended with severe tormina, or colic, or cramps at the stomach; or where the attack was sudden, and recent, and accompanied with fever, it was doing nothing to

prescribe small doses of hydrarg. c. cretâ and Dover's powder, which would produce their effect some days hence. It was expedient that the impression of the remedy should be in proportion to the force of the disease, and the rate of its progress. Accordingly, large doses of calomel and opium were given, to make the mouth sore immediately, or as soon as it could be done with safety.

“In several cases, in which the agony from tormina and tenesmus was extreme, and the evacuations were enormously frequent, and consisted altogether of morbid secretions, or blood, fifteen grains of calomel and two grains of opium were given at a dose.

“The patients, to whom so large a dose of calomel was given, were most attentively watched. Especial care was taken that nothing should divert it from its influence upon the constitution, and that every accidental inconvenience that might accompany its operation should be rendered as tolerable as possible. If the griping increased, they had peppermint water to have recourse to; if it still increased, they were to be largely and frequently fomented with flannels wrung out of warm water; and if it still increased, they were to be supplied with small doses of laudanum at short intervals.

“It will hardly be expected, that this single dose of calomel and opium could be effectual to the complete cure of the disease. The degree of relief which the patient experienced the next day, and the changes which his condition had undergone in the mean time, determined the manner of proceeding in the further treatment of the case.

“The dose of fifteen grains of calomel and two grains of opium, administered under such emergencies as have been described, had almost always the effect of calming the symptoms; but the degree of relief it procured was various.

“On the next day, we sometimes found that the patient had past an easier night, that the evacuations had been somewhat less frequent, and the tormina and tenesmus had been somewhat moderated, but that, since the morning, the symptoms had become worse again, the pains were as severe as ever, and the evacuations as frequent, and quite unaltered in their appearance. Under these circumstances, fifteen grains of calomel and two of opium were given a second time.

“ Sometimes, the day after the first large dose of calomel and opium, we found the relief, which had been procured through the night, still maintained, and the appearance of the evacuations changed, in some obvious respect, for the better. Perhaps they were now free from all admixture of blood, which they contained the day before. Under these circumstances, half the former dose of calomel and opium was given.

“ Sometimes, the day after the first large dose of calomel and opium, we found the patient exulting that he had been cured as by a charm; that he had slept all night, and his pains were gone; and that he had had several evacuations, of which the two or three last were almost natural. With this sudden improvement, salivation had either already arisen, or it was at hand. Under these circumstances, the use of mercury was either suspended altogether, or small doses of calomel and opium were given until ptyalism appeared, which was generally obvious at our next visit.

“ Our ultimate object, in all cases, was to produce salivation. But, in these cases of severer suffering, we found a salutary impression capable of being immediately produced by a few large doses, or even by one large dose of calomel and opium. This it was expedient to make the most of. Nevertheless, this immediate salutary impression was soon lost, unless the same practice was followed up to salivation; for which purpose mercury was afterwards sparingly or largely exhibited, according to the circumstances which have been set forth.”

In many, the process of cure was so gradual, that there was hardly any perceptible *action* engaged in it. “ The constitution seemed rather to lose the disease, one symptom after another, than to surmount it by an effort of its own.” In others, the process of cure was by a sudden, vigorous, and painful effort, when the constitution threw off its disease by a sort of critical paroxysm. “ Sometimes the critical effort commenced as soon as the mercurial factor was perceptible in the mouth. Sometimes salivation would exist 24 hours before the crisis began—and sometimes the crisis preceded the salivation 24 hours. But it never took place except where there was salivation at the time, or immediately before or immediately after.”

“ The critical effort was of this kind. After a calm, procured by one or two large doses of calomel and opium, or after

the employment of inunction for two or three days, the constitution would become suddenly roused, and a very severe griping would arise, and then a sensation would follow, as if the bowels were filling and distending themselves with something, and, afterwards, an incontrollable urgency to stool. With the evacuation came the relief of all the preceding misery. The stools were entirely changed. A few hours before they consisted, perhaps, of slime or blood, or some colourless turbid fluid. Now they were a colluvies of the foulest, blackest matter, and of every kind: heavy, ropy mucus and bile formed a considerable part of them. After one or two such evacuations the patient felt himself entirely restored and well. It generally happened, however, that the same sort of paroxysm returned, and was terminated by the same kind of relief. Thus, after a whole night spent in a succession of these critical paroxysms, the patients were found, the next day, bathed in a warm perspiration, and fast asleep; and, from this time, the evacuations from the bowels became natural and healthy."

"Before we resorted to the use of mercury for the various forms of the disease prevalent at the penitentiary, the state of the prison, in regard to that form of it which involved the brain and nervous system, was this:—Seven had already perished under our own observation; of whom one died apoplectic, one maniacal, two with the symptoms of phrenitis, two from cramps referable to the region of the stomach and the heart, and one from symptoms belonging in part to the heart and in part to the brain; and there were not less than two hundred now labouring under various degrees of disorder belonging to the same organs. Of them a few only were dangerously ill in respect of the magnitude of their symptoms. These few were suffering that insidious form of phrenitis already described, while we were checking the symptoms and postponing the progress of their disease, with little hope of eventually saving their lives; and, unquestionably, they were in great present peril. But all the rest, though not in imminent danger from the magnitude of their symptoms, gave just cause for anxiety, from the consideration that they had not been cured by any means hitherto employed, and moreover that they were under the same conditions of disease through which the seven had passed, before they reached their fatal consummation.

“Such was the state of the prison in respect to the disorders in question; and it may well be conceived that, under great apprehension for the event of all these cases, and under great present alarm for a few, we sought most anxiously for the means of their more successful treatment.

“At this time many patients, in whom mercury was first successfully employed for the cure of diarrhœa, were likewise freed from certain obscure nervous complaints; some from headaches, and some from vertiginous sensations. This occurrence, while it served to strengthen the belief that the flux of the bowels, and the nervous affections, had a natural alliance, and were, in some sort, the same disease, determined us to give the same remedy a fair trial in its application to both.

“And first, we most eagerly resorted to its use in those cases which occasioned us the greatest present alarm, viz. in three cases of insidious phrenitis. The patients already suffered subsultus of the tendons, and delirium, and one had strabismus. If life was to be rescued, it could only be by giving the remedy in such a manner as to bring the constitution as speedily as possible under its influence. Accordingly, as much calomel was prescribed, in repeated doses, and in combination with opium, as procured salivation in thirty hours; whereupon the most formidable symptoms were at once dissipated, and the patients were left in a condition favourable to recovery; and they eventually did recover.

“Next we resorted to its use in certain cases which occasioned us peculiar perplexity, and some apprehension of distant consequences, but no present alarm. There were many individuals in whom an affection of the head had been originally combined with bowel complaint. The bowel complaint had been very slight, and of short duration, and had ceased altogether during many weeks. But the affection of the head had been very severe; and, although it had obtained a few respites from common remedies, it still remained unmitigated; and at length all our medical expedients had lost the little temporary influence which they once possessed. There was every motive for trying the effect of mercury in these cases, and it was tried, and succeeded.

“One of these cases was peculiarly striking, and is well worth relating as an example. Upon our first visit to the peni-

tentiary, on the 1st of March, we found, among the patients in the infirmaries, a young man of the name of Robson. He was twenty-one years of age; and told us that he had suffered a headache of the most excruciating kind during several months. His sight was dim, and he had a constant twinkling of the eyelids, and great agony was depicted in his countenance. He had moreover that faded, pale, and melancholy aspect already alluded to. Yet the functions of his bowels were performed naturally. His tongue was clean, and his pulse was of the natural frequency and strength. At this time the alliance between the disorders of the nervous system and the bowels was hardly ascertained; therefore no inquiry was made whether he had suffered diarrhœa. Subsequently, however, we learnt that, during the preceding winter, his bowels had been twice slightly disordered for a few days. This poor fellow was under our constant observation and treatment, from the beginning of March to the end of June. He gained no respite from his agony, but by means of leeches applied to the forehead, day after day, for a week together, or by blisters kept open, or applied in quick succession behind the ears, or on the nape of the neck; and the respite thus obtained was of short duration: it might be for ten days; it was never longer than a fortnight; and then the same agony returned, and the same cruel treatment was to be resumed. In this case we eagerly resorted to the use of mercury; salivation was procured and maintained, to a certain degree, during several weeks. Whereupon the patient was released from his misery, and ever afterwards, during eleven months that I had the opportunity of watching him, he continued free from complaint of every kind.

“The beneficial influence of mercury upon that part of the disorder of the penitentiary, which belonged to the brain and nervous system, soon became as unquestionable as upon that which belonged to the bowels. It was proved upon the various forms, both of one and the other, and most conspicuously upon that form of each which occasioned us the greatest present alarm.

“Before the use of mercury, it was impossible to contemplate the state of the prison, and not consider an extensive mortality as inevitable. Experience of its effects during a fortnight entirely changed our anticipations of the result, and encouraged a hope that, with great care and vigilance in its administration,

the mortality would still be kept within narrow bounds. Thus, for every species of nervous complaint, as for every species of flux, whether they were combined or separate, (for either might occur alone, although they were generally found together,) we were led by our own experience to the employment of mercury.

“Seeing that the head-ache and vertigo were often the first symptoms, and that they often subsisted a considerable time alone before the accession of others, we thought it expedient to begin the treatment of the disease, as soon as it showed itself under this form, by our most efficient remedy. Our experience hitherto had been, that these affections of the head, when they were the first to declare themselves, were very seldom controlled, or in any way relieved by common remedies, and that, whether they were relieved or no, the flux almost inevitably followed. Treated with mercury, however, in the great majority of instances, they ceased; and, where they did cease under such treatment, in the great majority of cases, no flux followed.

“Thus did this remedy effect the relief of both disorders, when they appeared in combination, and of each, when either occurred alone; and, in the latter case, under such circumstances occasionally, that it seemed to prevent the accession of the other.”—*Medico-Chirurg. Review*, July 1825.

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### PATHOLOGY.

*Acute Cephalitis Simulating Hepatitis.*\*—M. H. a Dutchman, 24 years of age, of studious and sedentary habits, had been three years affected with hæmorrhoids, but had been otherwise healthy from youth. For twelve months past, this young Hollander had been deeply in love, but concealed his passion. In the beginning of February, he had an erysipelatous affection of the face, to which he paid little attention, and at this period, the discharge from the piles ceased. On the 17th of February, on returning from the theatre, he experienced a violent rigour, which lasted but a short time. Severe head-ache succeeded, with heat of skin, and copious perspiration. In the middle of the night the patient was seized with acute pain in the right hypochondrium, and difficult respiration. In the morning, our author and another physician found the patient in the following condition:—lies on

\* M. Bordot. *Revue Med.* Oct. 1824.

his back—very violent head-ache—heat moderate, and skin moist—*pulse full and quick*—acute pain in the region of the liver, which was very tender to the touch—tongue covered with a white coat—disgust for every kind of food and drink—had an evacuation in the morning—urine scanty and high-coloured—a slight erysipelatous swelling of the face on the right side. *Bled from the arm to 12 ounces—diluent.* At 4 P. M. the hepatic pain ceased—the breathing became free—the pulse weak—but the pain in the head continued—thirst. *Twelve leeches to the anus—pediluvium—emollient glyster.* 19th. At nine this morning, the patient seemed to be doing well—but the cephalalgia continued—he had made no water since the evening before—the erysipelas does not spread. (We do not think our author had much reason for concluding that his patient was doing well at this period.) After a slight slumber, it was found that the patient could not open his right eye, in consequence of the swelling of the eyelids. The urine had flowed, and was limpid—the pulse fell—complains of pain in the right side of the head—tendency to syncope. At seven in the evening he became delirious—restless—and made violent struggles to get out of bed—puts his hands often to his head—answers vaguely to questions—pulse quick and sharp—much perspiration over the whole body. He died at midnight.

*Dissection.* When the cranium was raised, about an ounce of sero-sanguineous fluid escaped—the vessels were gorged with blood—the ventricles were partly filled with reddish serum—the meninges were injected to a vivid redness—and there was a circumscribed spot of inflammation of the brain opposite to the right temporal bone, where a purulent exudation could be scraped off with a scalpel. Every other organ in the body was reported to be sound.

*Reflections.* “There are many maladies,” says M. Bordot, “that defy the resources of our art, and go on to a fatal termination in spite of all our remedies.” In this we agree with our author; but we do not think that this can justify us for using inert remedies in a dangerous disease. No man in this country will say that M. Bordot gave his patient any chance in the above case. An apparent inflammation of two important organs in a young man of 24, demanded other measures than 12 ounces of blood from the arm, some leeches to the rectum, and emollient

glysters. Such, however, is continental practice—and while it lasts, pathology must indeed flourish!—*Medico-Chirurg. Review*, July 1825.

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### MIDWIFERY.

*Cæsarean Operation.* In a late number of Graefes and Walther's Journal, the following curious case is related by Mr. Ruth, of Upper Silesia:—

Charlotte W——, ætat. 36, who had borne six children, became pregnant a seventh time, according to her own account, about three years and nine months previously to the date of report. She enlarged in size, and thought she could distinctly feel the motions of the fœtus. She became affected with unusual pains in the bowels, that at last were insupportable. She still continued to menstruate. Five or six weeks before the expected delivery, she strained herself, while washing clothes at a river, and felt something give way, at the same time a tumour suddenly formed a little below and on the right side of the navel. She became faint, and was carried home, where she remained confined for five weeks, experiencing a dull pain in the lower part of the abdomen. Labour pains came on, but gradually wore off, leaving the patient in a weak state. After this she resumed her domestic avocations, and continued to do so for upwards of two years, without any particular inconvenience. The menses were regular but scanty. In this situation she again became pregnant, and remained pretty well up to the 7th month, from which period the abdomen became very large and unwieldy, with great failure of strength. She was delivered, however, of a healthy child. Soon after the birth of this child, the mother became very ill, being thirsty, weak, afflicted with diarrhœa and hectic fever, which were rapidly hurrying her to the grave. The abdomen remained large, notwithstanding the general emaciation. There was a small painful tumour below the umbilicus. A surgeon laid this open, conceiving it to be an abscess. A quantity of matter and hair came forth. On introducing his finger he felt something hard. It was conjectured to be a fœtus, and it was at this time Mr. Ruth was called in, and found the patient in the situation described. The os uteri was close—the abdomen at the time had the appearance of a person at the full period of pregnancy, and through the opening abovementioned, the fœtus could be

distinctly felt. Mr. R. proposed an operation, which was assented to, and performed on the 31st December, 1822.

*Operation.* An incision was made in the linea alba reaching from above the umbilicus to within a short space of the pubis. The peritoneum was carefully opened, and the abdominal cavity exposed. A foetus presented itself, and was cautiously removed. The umbilical cord was traced over the uterus to the left side, where it was lost in a softened mass, probably the remains of the placenta. A great quantity of purulent matter was soaked up by a sponge from among the intestines. The uterus appeared larger than natural. The breathing becoming difficult, the wound was sowed up, and the patient put to bed, when an anodyne cordial draught was exhibited. The child was found to be a female, 18 or 19 inches in length, and well formed. Many of the external parts were in a state of suppuration—in other respects there was nothing particular in its appearance. The patient remained in a most precarious state for many days; but she gradually recovered, and, by the middle of March she was as well as ever.—*Medico-Chirurg. Review*, July 1825.

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### MEDICAL JURISPRUDENCE.

*Charge of Poisoning by Arsenic.* Messrs. Orfila, Vauquelin, and Barreul, were summoned by the Court of Assize, Department of the Aub, to ascertain whether certain matters found in the possession of the widow Laurent, contained arsenic. This widow had been accused of poisoning her husband ten days after marriage. The medical men who had examined the husband's body, came to the conclusion that the deceased was poisoned by arsenic; while, on the other hand, numerous experiments made at the laboratory of the Faculty of Medicine of Paris, could detect no arsenic in the matters submitted for analysis. The magistrates, however, were not satisfied, and M. Orfila was brought into court, when the following questions were put to him.

1st. Did the experiments made by those who first examined the body, prove that the matters taken from the stomach of Laurent contained arsenic?

2d. Is it possible to find, in the digestive canal of a person not killed by arsenic, grains, in appearance, of that mineral?

3d. What conclusion can be drawn from the circumstance of

a fowl dying after eating of the barley of which the deceased's ptisan was prepared?

4th. What conclusion can be drawn from the death of the leeches applied to the epigastrium of the deceased?

5th. Can it be concluded from the symptoms during the life and appearances on dissection of the deceased, that he died of poison?

1. *Answer to 1st Question.* The physicians charged with examining the body, in the first instance, state that they found in the stomach of Laurent a *pulverulent* substance which, when thrown on burning charcoal, *detonated, burned with a flame, and emitted the odour of garlic.* They hesitated not, therefore, to declare that the substance was white oxyde of arsenic. This conclusion was erroneous, for the oxyde of arsenic does not possess the property of detonating nor burning with a flame, when thrown on burning charcoal. As to the smell of garlic, it is not conclusive, since it might result from other substances than garlic, which might happen to be in the stomach. They should have dissolved the suspected substance in boiling water, and showed that a *green* precipitation took place on adding sulphate of ammoniacal copper, and a *yellow*, on the addition of hydro-sulphuric acid. This last precipitate ought also to have dissolved rapidly in liquor of ammonia.

2. *Answer to 2d Question.* The stomach of the deceased might have contained a granular substance possessing most of the properties assigned by the physicians to the supposed arsenical grains. Under certain circumstances, it is observed that the mucous membrane of the stomach and bowels is lined with a multitude of shining points, composed of fat and albumen. These, when thrown on burning charcoal, decrepitate while drying, and go off with a noise which might be designated, (though not properly,) detonation. These little bodies inflame like fatty matters, and exhale an effluvium of burnt animal substances. That these appearances are liable to lead to errors of a serious nature, the following instance is related to prove:—

On the 2d of August, 1824, the exhumation of an individual, 38 years of age, and who had been buried six weeks, was ordered by the *Procureur du Roi*. The lower extremity of the œsophagus, and the mucous membrane of the stomach and duodenum were found inflamed. A number of the small bodies, described

above, were found in the digestive tube, and were considered by the medical man appointed to analyze them, as oxyde of arsenic altered by animal matters. The substances, however, were transmitted to Paris, and analyzed by M. Vauquelin, who decided that they consisted of animal matters only, and contained no mixture of arsenic. Another instance, of a similar kind, is also stated by M. Orfila.

3. *Answer to 3d Question.* It is to be remarked, that Laurent died five days after eating an omelette, in which was the suspected poison. In this interval, he had taken several ptisans prepared by his wife. One of the physicians who visited the deceased, struck with the disagreeable savour of the ptisan, gave some of the grains of barley in it to a fowl, which fowl died the day following. A cat, who had devoured the entrails of the fowl, was seized with violent convulsions. These were puzzling facts, and we shall give M. Orfila's answer, verbatim.

“When oxide of arsenic is boiled with barley in water, the mineral is dissolved, and the liquid is rendered poisonous. The grains of barley, during this process, swell and imbibe the arsenical solution, and, when given to fowls, destroy them. But, on the other hand, if the barley water be first prepared, and then the arsenic infused and dissolved in it, the grains of barley, (having been swelled previously to the addition of the poison,) will not imbibe any more of that fluid, and consequently will be innocuous to fowls, *provided no grains of undissolved arsenic adhere to the barley.* These facts and considerations, justify the following answer, which I give to the question, namely. The arsenical oxyde, which, according to the act of accusation, was not introduced into the barley-water till after the latter was prepared, would remain dissolved in the fluid—and the barley itself would not be poisonous—*provided no particles of undissolved arsenic adhere to the barley.*”\*

4. *Answer to 4th Question.* The circumstance of the leeches dying soon after coming off the epigastrium, is quite undeserving of any serious answer. Every body knows that these ani-

\* With all due deference to M. Orfila, we think this was giving no satisfactory evidence at all. How could the court tell whether or not the fowl was poisoned by arsenic sticking to the barley? *Ed. Lond. Medico-Chirurg. Review.*

mals frequently die immediately after being applied to the bodies of patients, whatever may have been the nature of the maladies under which they laboured.

5. *Answer to 5th Question.* Laurent certainly evinced symptoms similar to those resulting from the exhibition of white oxyde of arsenic; but then there are numerous affections, in which we observe a similar analogy of symptoms, and where there can be no suspicion of poison. The ensemble of the phenomena, in the case under consideration, render it *probable* that poison had been administered. There are many instances on record where, although arsenic was actually administered, the usual symptoms did not follow—and *vice versâ*. Hence, where the mineral itself can be detected, the absence of the usual symptoms is no proof against the administration of the poison—while, on the other hand, the presence of the symptoms, without the actual detection of the arsenic, can afford no sufficient proof of poison having been given.

In illustration of the uncertainty attending the post-mortem appearances in cases of poison, M. Orfila stated to the court the following very curious history.

“ M——, aged 45 years, swallowed, in a paroxysm of passion, at eight o'clock in the morning, three drachms of white arsenic diffused in a glass of water. He immediately disclosed the transaction to one of his female friends, and rushed out of his house. Some of the arsenic remained in the glass, and was ascertained as such. The gentleman was searched for, in vain, during the space of two hours. At ten o'clock he voluntarily made his appearance. The imminent danger of his situation was represented to him—he acknowledged again that he had swallowed arsenic—and consented to take an emetic of tartarized antimony. The emetic had no operation. Large quantities of milk and mucilaginous drink were next administered, which had the effect of evacuating the greater proportion of the ingesta which had been taken. In one hour after this, (at 12 o'clock, or later,) the patient, who had previously suffered little or no inconvenience, complained of a painful sense of constriction at the epigastrium, accompanied by burning heat and thirst. His features became altered—the pulse was quickened. These symptoms became more and more intense—the parietes of the abdomen

were drawn towards the spine—the pulse became small and intermittent—and at five o'clock in the afternoon he expired.

“*Dissection.* No effusion in the abdomen—all the viscera of this region were in a natural state. The mucous membrane of the stomach and bowels presented no trace of redness, inflammation, or any change of structure. Oxyde of arsenic was found in the stomach and duodenum, in considerable quantity.”

Such was the evidence given by M. Orfila. The widow of Laurent was acquitted.—*Gazette de Santé.*

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### AMERICAN INTELLIGENCE.

*Aneurism of the Aorta.*—We have received a letter from our friend, Dr. HUGH J. OGILBY, of Madison, Ga. detailing the history of a case of aneurism, apparently commencing as high up as near the renal arteries, and descending towards the bifurcation of the aorta. The tumour was increased in size when the patient stood up. The right leg was of a much lower temperature than the left, which was easily distinguished by the application of the hand alone; there was also an evident diminution in the circulation in this limb. The patient was kept on low diet, and purgative and other antiphlogistics were persevered in for some time, and produced a perceptible diminution in the tumour, accompanied by marked improvement in the general health. He could stand up and move about without the inconvenience and suffering he had before experienced, and refused to continue the treatment because he felt so well. He was a temperate and industrious man, and in consequence of resuming his labours he soon fell a victim to his disease. His death was preceded by serous effusion into the abdomen.

We regret, with Dr. Ogilby, that he was not allowed to examine the body after death, as the appearances would no doubt have been extremely interesting to the pathologist. The time, we hope is not far distant, when the prejudice which bars the way to a more enlightened and useful acquaintance with the results of diseased actions will disappear, like most of the kindred superstitions which have already vanished before the increasing light of knowledge.

Our readers will learn with pleasure, that a System of Anatomy may be expected during the ensuing summer, from the Assistant Professor of Anatomy in the University of Pennsylvania, Dr. W. E. HORNER. We consider this circumstance as peculiarly advantageous to American students, inasmuch as this gentleman has long been a most assiduous cultivator of this science, and is justly ranked among the most skilful and profound anatomists our country affords. Having been for many years an assistant of the celebrated WISTAR, and for a long time since of his distinguished successor, he has enjoyed the amplest opportunities of advancing to the highest degrees of anatomical knowledge; having also largely contributed to the improvement of the splendid cabinet of anatomy belonging to the University, he comes well prepared to speak of all that is curious and interesting in this very important science. We shall be greatly mistaken if Dr. HORNER's work do not add much to the respectability of American professional literature, and we are quite sure it will hasten that period, (which we have prophesied to be near at hand,) when American students will cease to be dependent on European authors and compilers.

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Dr. SAMUEL JACKSON's work on *Practice*, we are happy to learn is now considerably advanced. That it is eagerly expected, it is unnecessary to state.

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A small and very useful volume has been recently published in this city, entitled *The Medical Formulary*, by BENJAMIN ELLIS, M. D. Lecturer on Pharmacy, whose talents and industry, added to correct professional judgment and minute pharmaceutical knowledge, have enabled him to give attractions to his subject, of which few would think it susceptible. The Formulary consists of an extensive collection of *prescriptions*, arranged according to the order adopted in Chapman's Therapeutics; they appear to be selected with much discrimination from the writings and practice of the first medical men in both hemispheres; the names of the articles comprising the formula are expressed in unabbreviated Latin, while the directions which follow are given in English; the utility of this plan is too obvious to require a comment.

The work is not loaded with dispensary formulæ; on the contrary, all the prescriptions are strictly *extemporaneous*, and

such as every physician should be more or less familiar with. A few medical and pharmaceutic remarks are interspersed through the book, at the same time that tedious details, and especially theoretical inquiries, are carefully omitted.

When it is recollected that most young physicians are in the custom of keeping manuscript records of prescriptions, and also that gentlemen just entering upon their professional duties have no analogous work to which they can recur with confidence, we believe that this volume will meet with a cordial welcome from the medical public. We would more especially recommend it to our brethren in distant parts of the country, whose insulated situations may prevent them from having access to the many authorities which have been consulted in arranging materials for this work.

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The first number of a new periodical, entitled "The North American Medical and Surgical Journal," has recently been published in this city, conducted by several gentlemen distinguished for their acquirements and zeal, whom we are happy to rank among our most respected friends. To their arduous and honourable enterprize, we wish all prosperity, and we cannot but rejoice at the principles they have declared their intention of being governed by, since they are the same which we have unremittingly adhered to since the commencement of our career; an undeviating attention to justice and impartiality, and an entire rejection of every thing suited to produce discord and injury to the profession. We are happy to anticipate in this new journal a strong coadjutant, whose especial object is that of eliciting the best exertions of our professional brethren, and placing the profession itself on that high ground, which its usefulness and excellence so justly entitle it to hold in the estimation of mankind, but of which it has been too long deprived by the malignant and unworthy conduct of many who have disgraced its ranks. But the time is near at hand, when such miscreants will be silently delivered over, by the withering contempt of the *profession*, to the influence of *public scorn*: *MEDICINE*, freed from such disgraceful incumbrances, will then appear as she is, the benefactress of mankind, and those who wear the garb of her votaries, will be not less distinguished for their talents, assiduity and skill, than for the possession and exercise of every

moral attribute which can confer attractiveness on virtue, or add dignity to the human character.

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We have received a letter from Dr. BAKER, of Washington, D. C. (which we should insert entire but for want of room,) relative to his letter on DYSENTERY, which we copied into our last number from the *National Intelligencer*, and prefaced by some remarks relative to the impropriety of publishing professional papers through the *commercial* press. We are perfectly satisfied that Dr. BAKER's motives were correct, and have no doubt that the publication of this letter in the newspaper, formed no part of his inducement for writing it.

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In the 16th and 20th Numbers of this Journal, an extraordinary case is related, in which clots of blood, flesh, pieces of cartilage and bone, oozed from beneath the eye of a girl 13 years old. Whatever may be thought of the credibility of this story, and certainly it requires no little effort of credulity to believe it, cases not altogether dissimilar are related by respectable authors. In the work of GUERIN on the diseases of the Eye, cases of this nature are noticed. "It is in this place," (between the globe of the eye and the eyelids,) says Guerin, "that those stones were formed, which were seen to come from the eye of a child ten years of age.\* A case is related in the *Journal des Savans*, where stones came out at different times of various sizes, one as large as a bean. This story, which appears suspicious, was verified at the time, first by a lady who took care of the child, and afterwards by Mr. d'Emery, and by Messrs. Scorbiac and Van-helmont, celebrated physicians; they were astonished and convinced of the fact, which appeared to them very surprising.

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We have read with great satisfaction the valuable Introductory Lecture, delivered in November last, at Columbia College, D. C. by THOMAS SEWALL, M. D. Professor of Anatomy in that Institution. His subject, the History of Medicine in this country, is one of deep interest, and by his judicious mode of viewing it, it is rendered still more attractive. It was our wish

\* *Traité sur les maladies des yeux par M. Guerin, ancien chirurgien en chef du Grand Hôtel Dieu de Lyon, &c. &c. page 40.*

to have extracted some of the many interesting facts contained in this lecture for the benefit of our readers, but as we have been obliged to leave out several articles for want of room, we must refer to the lecture itself, which will amply repay the reader for the time employed in its perusal.

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## MEDICAL INSTITUTE OF PHILADELPHIA.

(EIGHTH COURSE.)

The Summer Lectures will begin the second week in April next, and be continued till the first day of November, August excepted.

NATHANIEL CHAPMAN, M. D.

On the Practice of Medicine.

WILLIAM P. DEWEES, M. D.

On Midwifery.

WILLIAM GIBSON, M. D.

On Operative Surgery.

SAMUEL JACKSON, M. D.

On Materia Medica.

WILLIAM E. HORNER, M. D.

On Anatomy.

JOHN R. MITCHELL, M. D.

On Chemistry.

JOHN BELL, M. D.

On the Institutes of Medicine and Medical Jurisprudence.

HUGH L. HODGE, M. D.

On the Principles of Surgery.

W. E. HORNER, M. D. *Secretary.*

21st January, 1826.

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